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No. 2576

United States

Circuit Court of Appeals

For the Ninth Circuit.

Transcript of Record.

JAMES B. SMITH, F. C. MILLS and E. H. MAYER,

Plaintiffs in Error,

vs.

THE UNITED STATES OF AMERICA,

Defendant in Error.

VOLUME V.

(Pages 1537 to 1920, Inclusive.)

Upon Writ of Error to the United States District Court of the Northern District of California, First Division.

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(Testimony of Edward Park.)

Q. Calling the four tubs a round. Mr. Mullan has so testified, Mr. Wilson has so testified, and so have a number of other witnesses for the defense testified that it was the custom to weigh four tubs at one time, one after another?

A. They only weigh four tubs at a time, and only four—sometimes only three—when the barge is stopped, we will say, the chute is blocked up, and the tubs are remaining there for ten minutes or a quarter of an hour; then if those tubs are even, not overloaded, then the weigher may possibly take four, but sometimes he will take three.

Q. Isn't it a fact, Mr. Park, that it has been the custom, almost invariable custom, for the custom-house weigher to call for four tubs to be placed upon the scales at a time and those tubs were weighed consecutively, one, two, three and four, before another tub unweighed was deposited into the bunkers of the ship? A. No.

Q. Has it ever been done? A. It has been done.

Q. Isn't it the general thing to do it?

A. No, certainly not.

Q. About how often is it done in, say, a working day? **[1342—1280]**

A. Well, as I said, if the barge is stopped, and those tubs are in the barge filled, and those tubs are correct, not overloaded, then they might weigh them; but not otherwise.

Q. Isn't it the practice, the usual practice, for the custom-house weigher to say, "On the scales," when he desires to weigh a tub, and tub No. 1 is weighed,

(Testimony of Edward Park.)

and then tub No. 2 is weighed, and then No. 3 is weighed, and tub No. 4 is then weighed, and thereafter the tubs are sent unweighed into the ship for a certain length of time, and then they go through the process again of weighing 1, 2, 3 and 4?

A. No, they will take one tub and weigh that, and then let two or three tubs go by, and then take another one, and in some cases they will take two tubs at a time, provided that they are not overloaded.

Each tub retains permanently the number given to it. The numbers are painted on the tubs or put on with chalk. Of course, a tub is occasionally out of order, and, in such instances, we might change its number. Otherwise, it retains its number the year through. Generally, after a tub has been out of order, it is given its own number when it comes back. Tubs are weighed for the purpose of ascertaining the tare sometimes twice a day if it is raining; once a day always. The tare is ordinarily taken when we commence weighing in the morning, but that depends entirely on the weather. If it rains in the afternoon we take a second tare then. I would not swear to the fact, but I would say that in all probability the various barges have on them the same tubs as in 1912. The tubs are of heavy iron and the rain sometimes leaves a kind of coating of fine coal on them. When we take a tare, however, we get the shovelers to clean the tubs off. Fine coal will stick to the tubs in spite of the rain unless **[1343—1281]** we do so.

Q. I see by your book here that you weigh each tub generally the same number of times; is that a

(Testimony of Edward Park.)

fact? For instance, in the case of the barge "Nanaimo" weighing into the "China," March 6, 1912, there were nine weights taken of each tub, one, two, three and four. Now, is that always the custom?

A. The custom for what?

Q. Is it always the custom to weigh each one of the four tubs the same number of times, that is, the tubs that are weighed for the purpose of fixing the average?

Mr. OLNEY.—Show him what you refer to, Mr. Sullivan.

Mr. SULLIVAN.—Q. I will ask you according to your recollection now?

A. Well, we weigh about the same number of times, but we don't weigh them all together.

Q. You don't weigh them all together?

A. No, we don't.

Q. Now, if you weigh No. 1 tub one time, do you mean to say that you will wait a little while and then weigh No. 2, and then wait another little while and weigh No. 3, and then another little time and weigh No. 4, or do you do this: Do you weigh No. 1 once, and then, after a given number of weights are taken, do you weigh No. 1 again?

A. They do not always weigh the same way; sometimes they will take No. 1 tub, and very likely they will take No. 4, when No. 4 comes up; they don't weigh them in rotation, one after another.

Q. After weighing No. 1 tub, do you weigh that No. 1 tub again before you weigh all the other tubs?

A. In many cases they will weigh No. 1 perhaps

(Testimony of Edward Park.)

twice, sometimes weigh No. 2 twice, and weigh perhaps No. 4, and No. 3 is not weighed; they vary, they don't always do the same. Their object **[1344—1282]** is to get a fair and square weight.

Q. Well, I will pick out the numbers here at random. Take the case of the "Wellington" discharging into the "Korea."

Mr. KNIGHT.—What date?

Mr. SULLIVAN.—February 16, 1912.

Q. Now, you see each tub is weighed the same number of times, isn't it?

A. No, that tub No. 1 is weighed once.

Q. No. 1 is weighed once?

A. You have got to multiply it by the number of tubs to get the weights.

Q. Take No. 2, 3, and 4, five times, isn't it, each one?

A. Yes, we took the same number of tubs, according to the orders of the custom-house weigher.

Q. Take another one at random here, say March 6, 1912. Now, weren't each one of those four tubs weighed nine times? A. Evidently, they were.

Q. Do these numbers up here, numbered 1, 2, 3 and 4, indicate that those four were weighed in a round, or weighed at separate times?

A. I could not swear to that right now.

Q. Look at your book, here, and, considering that each tub *was a* certain number of times, nine times, would you not say there was a round of tubs weighed at that particular time?

A. No, they might take them any way.

(Testimony of Edward Park.)

Q. We will take another one at random, here. Take the case of the discharge of the "Comanche" into the "Mongolia," April 5, 1912, was not each tub weighed five times on that occasion, that is, tubs numbers 1, 2, 3 and 4.

A. Each tub at different times had to be weighed, according to the number.

Q. Was not each tub weighed five times, that is, tub 1, tub 2, tub 3, tub 4—were they not each weighed five times? A. Evidently so. **[1345—1283]**

Q. Now, take another one at random, here. I pick out here October 2, 1911, and ask you if each tub was not weighed four times?

A. 16 weights were taken.

Q. Was not each tub weighed four times?

A. At different times.

Q. I show you another weight, taken on September 3d—is that 12 or 11? A. 11.

Q. Was not each tub weighed eight times on that occasion?

A. Yes, there was 32 weights taken; there had to be, on account of the number of tubs.

Q. Now, then, how does it happen that you weighed No. 1 once, and then weighed No. 3 another time, and then 1 again—how does it happen that you did that, when this gives you the weight of each tub the same number of times?

A. We have got to weigh them. We have got to get the weights, one in fifteen in each one.

Q. Now, I will show you a case of the discharge of the "Nanaimo" in to the "Manchuria," October 13,

(Testimony of Edward Park.)

1911, were not there six weights of each tub taken on that occasion?

A. Yes, those tubs appear that way.

Q. I will take another one here, September 15, 1911, and I will ask you if on that occasion, when the "Comanche" was discharged into the "Siberia" there were not seven weights taken of each tub on that occasion?

A. That all depends on the number of tubs—yes, there was. That is correct, 28 weights were taken, which the custom-house demands.

Q. Now, the practice is to add up the total of the weights, the four weights, in this case? A. Yes.

Q. And then—

A. (Intg.) Divide that amount by seven.

Q. Divide it by seven. That gives you the average weight of the coal in that tub, does it not?

A. Yes. **[1346—1284]**

Q. Then you deduct the tare? A. Yes.

Q. Which leaves the average weight?

A. The net.

Q. The net average weight of the seven tubs.

A. No, no; it leaves the net of No. 1.

Q. I mean of No. 1, that particular tub?

A. Yes.

Q. You find the four net weights, and then you add up the four net weights, don't you?

A. We add up the four net weights, and multiply by the number of tubs, provided they are equal.

Q. To find the gross weight of the coal?

A. To find the net weight of the coal; but they have

(Testimony of Edward Park.)

got to be equal tubs before you can multiply them by that.

Q. (Mr. KNIGHT.) That is, you mean there have to be equal weights taken of the tubs?

A. Yes, the tubs have got to have the same number of weights before you can multiply them.

Mr. McCUTCHEN.—That is to say, the same number of tubs of each number? A. Yes.

Mr. SULLIVAN.—A. Now, I see here in one of these books that on October 19, 1912, you discharged "Commanche" into the "Siberia." Didn't you on that occasion weigh each tub the same number of times? A. Thirty-two weights were taken.

Q. Now, this represents the correct tare of the tubs, don't it, on that date? A. Yes.

Q. Now, tub No. 1 on the "Comanche" on that date weighed 615 pounds, did it? A. 615.

Mr. KNIGHT.—You are speaking of the net weight, Mr. Sullivan?

Mr. SULLIVAN.—No, the tare of the tub.

Q. What is that date? A. October 12.

Q. Now, will you look at September—that same tub was in use, was it not, in September and October of that year? **[1347—1285]**

A. Well, I couldn't swear to that.

Q. Well, now, will you see what the tare of the tub was, according to this book here, on September 15?

A. It was not in the same month—the tare varies.

Q. Is that 650 or 630? A. It is 630.

Q. The tare of that tub in October is 615, is it not?

A. Yes.

(Testimony of Edward Park.)

Q. What was the tare of the tub No. 2 in October, 1912—was it not 620?

A. If it is down there 620, it was 620. The tubs vary repeatedly.

Q. Well, that is tub No. 2 on that date. Was not that same tub on the "Comanche," too, in September?

A. Well, there is a month's difference. They tare them every day.

Q. You take the tare of the particular tubs every day, but was not that particular tub No. 2 on the "Comanche" in the month of September and in the month of October?

A. Well, if it is in the book, there, whatever the tare is in that book, is correct, according to the custom-house officer.

Q. This tare here, October 19, was correct, 620?

Mr. KNIGHT.—They are a year apart. That is 1911.

A. This is 1911 and the other 1912.

Mr. SULLIVAN.—Q. All right.

A. You know the tubs vary.

Q. Were not the tubs on the "Comanche" in use a full year without being changed at all?

A. I would not swear to that.

Q. I show you the weight of the tub No. 3, on the "Comanche" on Septetmber 15, 1911; it was 585 pounds, was it not?

A. If it is in that book, that is it.

Mr. KNIGHT.—Q. Will you verify that?

A. 585, No. 3, **[1348—1286]** "Comanche," September 15, 1911.

(Testimony of Edward Park.)

Mr. SULLIVAN.—A year later, it was 550, was it not? A. I would not swear to that.

Q. On your book, here.

A. These weights agree with the Government weigher's.

Q. I know, but according to your book, here, it weighed only 550 pounds in October, 1912; isn't that right?

Mr. KNIGHT.—That is assuming, Mr. Sullivan, that it was the same tub.

A. I would not swear they were the same tubs; they often get out of repair, very often, and are patched up and fixed.

Q. Now, I show you in this book, on September 15, 1911, the tare of tub No. 4, on the "Comanche," was 770 lbs., was it not? A. 770.

Q. According to this book here of October 19th, 1912, the tare of that same tub was 660 lbs., was it not?

Mr. KNIGHT.—Of couse, Mr. Sullivan, I suppose it is understood that that means the tare of tub No. 4?

A. I couldn't swear to that.

Mr. McCUTCHEN.—And counsel is consistently putting into his question the suggestion that the same tub 13 months afterwards weighed so much and the witness has dozen times said that he cannot tell.

* * * * * * * *

For instance, very often, if you take on a small barge, such as the "Nanaimo," she will sometimes break a tub. They don't always bring a small tub to replace it, they will bring a large tub, a tub that is

(Testimony of Edward Park.)

perhaps 150 lbs. or 100 lbs. larger, a large tub from the bunkers. Very often that is done, in a hurry to get a tub, they will send one from the bunkers which is a larger [1349—1287] and a heavier tub than the others.

Q. This book shows—

A. (Intg.) This book shows it exactly as the custom-house weigher gave it.

Q. This book shows that tub No. 4, on the "Comanche," weighed 660 lbs., does it show?

A. That shows, on the 19th of October, 1912, that that weighed 660 lbs.

Q. And this book which I have in my left hand, bearing date September 15, 1911, shows that tub No. 4 on that date weighed 770 lbs., or 110 lbs. more; is not that the fact?

Mr. BLACK.—That is not the same tub.

Mr. SULLIVAN.—Well, we don't know whether it is, or not.

A. Well, I would not swear that that was the same tub. It possibly might have been a new tub.

Q. Do you think that the rain would swell the tub to such proportions that it would lose 110 lbs. in a year?

A. They might change the tub; that is often done. We often on those barges change the tub. They break down and in the hurry they send another tub. Sometimes they have extra tubs on the barges and sometimes they have not; they have to send down to the bunkers for them. But the Government weighers, the experience I have had with them, they are

(Testimony of Edward Park.)

the most careful men I have ever seen.

Q. The most careful men in the universe?

A. I didn't say in the universe. They attend to their business. I have had men with me for years that were just as honest and fair and square, and did what was right; I would not wish to have better men. That was my experience with them for years. Men of experience, men who have been weighing coal for years.

Q. Now, I take up another book here, dated April 4, 1912, I [1350—1288] think it is, is it not?

A. Yes, that is April 4, 1912.

Q. What is that—is that Wellington? A. Yes.

Q. Wellington coal ex "Theobold"?

A. Yes sir, April 4, 1912.

Q. Now, I will ask you, Mr. Park, when that boat was discharged if they didn't weigh each one of the tubs, 1, 2, 3 and 4 the same number of times, eight times each?

Mr. KNIGHT.—When you speak of the witness weighing the tubs, Mr. Sullivan, you mean the custom-house officers, I suppose?

Mr. SULLIVAN.—Yes. He takes part in the weighing.

Mr. KNIGHT.—He checks off.

A. I don't touch the beam, I don't touch the scales at all.

Mr. SULLIVAN.—Q. You look at the scales though and make a note of the weight?

A. Yes, certainly I do that.

(Testimony of Edward Park.)

Q. Do you wear glasses when you are taking the weight?

A. No, sir. I wear glasses just at a distance; my eyesight I am happy to say is good.

Q. When the weight is being taken you look always at the scale, do you?

A. I always look at the scale.

Q. You always look at the scale, do you, and without glasses?

A. Yes, repeatedly. Sometimes on a dark day I might put my glasses on to look at the scales.

Mr. McCUTCHEN.—Mr. Sullivan, do you claim that there is any difference between this record and the record of the custom-house weigher?

Mr. ROCHE.—Of course not. The witness testified that they compared notes and saw that they tallied before they left there at night.

Mr. SULLIVAN.—Certainly. Somebody would lose his job if they didn't agree.

Q. How old a man are you, Mr. Park, if you are not ashamed to [1351—1289] answer the question? A. Well, what do you think?

Q. Oh, about 70? A. No, not quite that.

Q. Close on to it, aren't you?

A. Well, 65 or 66.

Q. Will you say that on this occasion these tubs were not all weighed—that is, tub No. 1, if there was not a round of tubs, 1, 2, 3 and 4 weighed first at one time? A. Oh, I couldn't tell that now.

Q. Well, look at your book there?

A. That don't show anything. I would not swear

(Testimony of Edward Park.)

to that because the custom is to weigh the tubs at different times, and at any time, and to take any tub; you don't take them in rotation, you take any tub; you might take tub No. 4, or Tub No. 3, or tub No. 2, but you have got to get your 15 weights in.

Q. You are still positive that there is not a round of four tubs weighed, as a general thing?

A. I am telling you the truth, just exactly as we have done it, and I am telling you that sometimes they will weigh four, provided they are not overloaded. The custom-house officers know what coal is; they have been at it all their lives, and they are remarkably fine men; I have not a word to say against one of them; every one of them knew their duty, and I don't think I had a word with any of them. On many steamers we would never have a single word because they knew what they were doing perfectly well.

Q. When weighing on the scales, do you always get the exact number of lbs. of the tub and contents?

A. As near as possible.

Q. What do you call as near as possible; within how many lbs. would you say is as near as possible?

A. Sometimes they will weigh within 5 lbs., give and take. **[1352—1290]**

Q. How do you weigh, upon a rising beam or a falling beam? A. Halfway between.

Q. Even beam?

A. Even beam; sometimes it may be just a fraction, but it is so near even that it is about even.

Q. And you say it varies about 5 lbs. either way,

(Testimony of Edward Park.)

or it might? A. Yes, sir, give and take.

Q. I show you here in this book showing the discharge of the barge "Nanaimo" into the "Korea"; each tub was weighed six times was it not?

A. Yes, sir.

Q. Now will you explain this phenomenon, how tub No. 4 weighed just exactly 1,600 lbs. the first three weights taken, the tub and contents?

A. That happens very often.

Q. And how it happens that the next two tubs weighed just even 1570 lbs.?

A. That happens very often indeed, when it is light coal, like the Japanese coal, it very often runs that way. Sometimes we have three tubs that weigh exactly the same; that comes very often.

Q. You say that comes very often?

A. Oh, yes, it does. If you will look in the book there you will find any amount of them that are so close that there is hardly 10 lbs. between.

I would say that a shovelful of coal would weigh about 200 pounds.

Q. Will you explain this phenomenon here, appearing on this page here, showing the discharge of the Wellington in August, 1913; the fourth tub, weighed twice, weighed 2240 lbs., just an even ton; the next two weights were 2250 lbs.; two of them weighed just an even ton right down to the shovelful? A. That is correct. **[1353—1291]**

Q. How did it happen, do you know?

A. Because it weighed exactly that.

Q. Because it weighed exactly that?

(Testimony of Edward Park.)

A. That agrees exactly with the Government weigher, and that is the weight that it was exactly.

Q. Was there any guessing done about that time by either or the custom-house weigher?

A. I don't do any guessing.

Q. Does the custom-house weigher do any guessing in making these entries?

A. Those men don't do any guessing; those men know their business.

Q. That is August 20, 1913, isn't it?

A. Yes, August 20, 1913.

Q. Get me a discharge from the "Wellington" in this book, will you?

A. This is from the "Wellington" to the "Korea."

Q. What is the date of it? A. December, 1912.

Q. This one is August, 1913, here, and this is December, 1912? A. December 24, 1912.

Q. Now, on December 24, 1912, the tare of tub No. 1 on the barge "Wellington" was just 720 lbs., was it not? A. That is correct.

Q. And in August, 1913, the tare was 810 lbs., the tare of tub No. 1?

A. No. 1 was 810 lbs.; that was August, 1913.

Q. Can you explain how that tub increased in weight 90 lbs., during that period of time, if it happened to be the same tub?

Mr. KNIGHT.—That is objected to upon the ground that it is the same bucket, and the witness has said repeatedly that he could not state whether it was the same or a different tub.

(Testimony of Edward Park.)

The COURT.—That is true.

A. I could not swear to that at that length of time apart; it [1354—1292] might have been repaired. I could not swear it. They changed the tubs so often when they are out of repair.

Mr. SULLIVAN.—Q. In August, 1913, tub No. 2 weighed 700 lbs., did it not, tub No. 2 on the "Wellington"? A. Yes, sir, 700 lbs.

Q. And on December 24, 1912, tub No. 2 weighed 725 lbs.? A. No. 2 weighed 720 lbs.

Q. Is that 720? A. Yes.

Q. I think I made a mistake before. Tub No. 1 on December 24, 1912, the tare of tub No. 1 on that vessel at that time, was 710 lbs., was it not?

A. On December 24, 1912, the tare of tub No. 1 was 710 lbs.

Mr. McCUTCHEN.—These comparisons are all more than a year apart, Mr. Sullivan.

Mr. SULLIVAN.—One is August, 1913, and the other is December, 1912.

The WITNESS.—But you can't tell about that, because the tubs are broken down and are repaired very often, very often indeed.

Q. The tubs on the "Wellington" are all the same shape, are they not?

A. Oh, they are all about the same shape.

Q. And the same size?

A. Very near; there is a slight variation.

Q. The tubs on each of the other barges are of the same size, are they not, and approximately the same weight?

(Testimony of Edward Park.)

A. Yes, sir, the tubs on the smaller barges are about one size.

Q. The purpose of the company is to have the tubs as nearly equal as possible, on each barge, as to carrying capacity, is it not?

A. Yes, sir, but very often a tub will break down and they will send another tub to take its place. [1355—1293]

Mr. KNIGHT.—Just a moment; we object to that, if your Honor please, asking what the purpose of the company is. The question is here, what was the fact?

Mr. SULLIVAN.—Q. As a matter of fact, Mr. Park, the tubs are of nearly the same weight on each barge, are they not? A. Very nearly.

Q. And as a matter of fact, the tubs on each barge contain approximately the same quantity of coal when filled to the water line, as you call it?

Mr. McCUTCHEN.—That would depend on many conditions, Mr. Sullivan; it would depend on the character of the coal?

A. Certainly it does. If it is Japanese coal or if it is Comax coal, there is a great difference.

Mr. SULLIVAN.—Q. Well, the cubical contents of each tub on each barge is about even?

A. Pretty nearly so.

Q. And the tubs are all made of the same material?

A. They are made of iron or steel; some are steel are iron.

(Testimony of Edward Park.)

Q. Those on the "Wellington" are all iron, aren't they?

A. I would not swear to that; they are either iron or steel, I would not swear which, I couldn't do it.

Very frequently I would work overtime. I remember being on one of the barges on December 18th, 1912, when the "Korea" was being loaded. We worked all night. I remember testifying on direct examination that the tubs were evenly filled on that occasion. The barge was the "Wellington." I believe it was another barge that worked in the daytime in the coaling of that vessel. The barge discharging on the day preceding the night of the 18th, according to my books here, was the "Theobold." I was presumably taking tally then. I was, to the best of my **[1356—1294]** belief, on duty during the entire period from the time when the "Theobold" commenced discharging up to the time the "Wellington" finished discharging the next morning. I am not quite sure, however, about the "Theobold." I am certain I was on the "Wellington." The entries of discharge from the "Theobold" are in my handwriting. The custom-house officer commenced at the usual time, 7 o'clock, on the morning of the 18th. I was on duty then. The entries showing the discharge of the "Wellington" at night into the "Korea" are also in my handwriting. I was therefore on duty nearly 24 hours. That does not occur very often, but sometimes. On such occasions I would sleep in the chair between 12 and 1 o'clock at night. The men go to a restaurant, but I stay on

(Testimony of Edward Park.)

the barge, bringing my lunch with me. The entries at 5 o'clock on the morning of the 19th appear to be as well written as those of the previous day. They are in the same hand as the others. I wrote them all. Four weights were taken that night.

Q. I see the tub 1 is 710 lbs., December 18, 1912?

A. Well, whatever is in that book is correct because I had one of the finest men on the force with me—I had two of them.

Q. Who was that?

A. Mr. Finnegan was one of the finest on the force.

Q. And is he still on the force?

A. You bet he is.

Q. And he has been on for many years?

A. Yes, sir, and he is a cracker-jack.

Q. He is a cracker-jack, is he? A. Yes.

Q. He knows how to weigh coal, does he?

A. You bet your life he does. I am very happy to say that the weighers I have had with me during my term with the Pacific Mail Company were A No. 1 men. If they had not been they would have been reported in **[1357—1295]** about 5 minutes to Mr. Chisholm.

Q. Oh, Mr. Chisholm is your boss, is he?

A. Yes.

Q. He is another fine man, isn't he?

A. Well, I have nothing against him. I have found him right at the steamer repeatedly, both in the daytime and the night, and if anything was wrong I wouldn't be there 5 minutes.

(Testimony of Edward Park.)

Q. You always get along very agreeably with those weighers, don't you?

A. Yes, sir. Why shouldn't I? They did their duty and I tried to do mine and between us I don't think there was any trouble.

When I worked all night on the 18th, I do not think I used my glasses to see the weights; I only use them when it is dark or steamy. My eyesight is very good. Three rounds of weights were taken up to 11 or 12 o'clock. The number of weights taken from 1 to 5 in the morning would be shown in a different place in the book. I think it was somewhere in the neighborhood of 5 or 6 o'clock when the "Wellington" quit discharging coal into the "Korea" on the 19th. It happens right through the book that tubs will weigh precisely the same number of pounds; that does not occur frequently, however. When the "Korea" and "Siberia" were being coaled, we usually worked at night. I do not know how many times I worked at night during the year 1913. We particularly have to work at night when the vessel is listing, and we have to even her up. I sometimes work from 22 to 24 hours when the "Korea" and "Siberia" are being laden with coal. The "Korea" is due about every two months, and the "Siberia" with about the same frequency. Once in a while we work all night on other vessels when a steamer is late coming in.

I always used to make out reports of the weights for the Western Fuel Company. The custom-house weigher does not do so. There is sometimes, but not

(Testimony of Edward Park.)

always, a tally clerk for the [1358—1296] Western Fuel Company on hand when we are coaling the Pacific Mail ships. The Western Fuel Company, however, relies on my report. I give these reports to the Western Fuel Company's foreman when he comes around. The Company has given me compensation for this service. That would be at Christmas-time, and that is the only time. They have never paid me any overtime. I think I have been receiving this compensation every Christmas for the last eight years, but there has been no compensation between Christmases.

Q. By the way, have you ever learned, while representing the Pacific Mail Company in seeing these weights taken, have you ever heard of complaints or learned of complaints that the barges almost invariably discharged more coal than went into the barges?

A. No, sir, I did not hear those reports.

Q. You never heard—

A. Well, I might have heard it once, but I have known many times when the clerk would come, and then they would tell me that the barges were 30 or 40 tons short.

Q. Many times? A. Well, repeatedly.

Q. Did you hear that the barges were 30 or 40 tons short more often than you heard that the barges were 30 or 40 or 100 tons over?

A. Well, I never knew the amount of coal that was in the barge when it came.

Q. Didn't you hear, in the performance of your duty, or did you not learn, that in 90 or 95 per cent

(Testimony of Edward Park.)

of the cases the barges turned out a great deal more coal than went into the barges?

A. I cannot say that I have.

Q. Did you hear on more than one occasion that the barges turned out more coal than they had received? **[1359—1297]**

A. Well, I cannot say that I have; I cannot remember that I ever heard that the barges ran over. It was never reported to me.

Q. And you never heard it at all?

Mr. KNIGHT.—I submit, if your Honor please, that this hearsay information is not relevant. It calls for hearsay information on the part of the witness.

The COURT.—Of course that fact itself would not be relevant, but it might be a matter that would cause him to look more carefully into his weights.

Mr. SULLIVAN.—That is the theory upon which we asked the question.

A. When a barge comes to the dock I don't know the amount of coal that is in that barge until it is weighed. I am not told how much coal is in the barge. I don't know.

Q. Don't you know, as a matter of fact, from common rumor about the place where you are employed daily, that the barges almost invariably turned out more coal than they received?

*   *   *   *   *   *   *   *

A. Well, I would not swear to that. I may have heard it; I cannot say. I don't remember hearing

(Testimony of Edward Park.)

it, that the barges were turning out more, only lately, just lately.

Mr. SULLIVAN.—Q. That is, since these indictments were found. How often did the clerk report to you that the barges were turning out short, turning out less coal than went into them?

Mr. KNIGHT.—What clerk?

Mr. SULLIVAN.—He said some clerk reported to him.

A. Well, I remember distinctly when Mr. Mills was away sick that Mr. Eddie Powers came down to the dock with a statement and he told me that the barge was—I would not swear now to the amount, but it **[1360—1298]** was within 30 to 40 tons short, one barge.

Q. Is that the only time you ever heard complaints made, that the barge was turning out short?

A. No, sir; I have heard it at other times, that the barges were short in turning out.

Q. How often would that be?

A. Well, I could not exactly say. I have heard that the barges were turning out short.

Q. From whom did you hear that the barges were turning out short?

A. Well, I have heard from Mr. Mills two or three times that the barges were turning out short.

Q. And Mr. Mills complained to you about the barges turning out short did he?

A. Well, he told me about it, and I referred him to the weigher.

Q. Did he tell you not to let that occur again?

(Testimony of Edward Park.)

A. Nothing of the kind.

Q. But he was complaining at the time?

A. He told me that the barges had turned out short.

Q. And did Mr. Mills ever tell you that the barges turned out long or over?

A. He never told me anything about that.

Q. He never told you anything at all about turning out overages?

A. He told me repeatedly to keep my tubs even and that if any coal went overboard, to take as many tubs as I wanted to make the thing square.

* * * * * * * * *

Q. Did Mr. Mills make the statement about the barges turning out short in the presence of the hatch-tender?

A. Well, it was on the barge, so I presume that it must have been; they were all around together.

Q. Don't you know that the engineers of the Pacific Mail were contending and complaining, while you have been in the service **[1361—1299]** of the Pacific Mail, and for the last eight years, that less coal went into the ships under their charge than the records of the Western Fuel Company showed?

A. No, sir, I do not know that; I have had no complaint from the weighers, only I think two.

Q. I mean the engineers of the Pacific Mail Steamship Company, that they were being short-weighted in the coal that went into their ships?

A. Only on one or two occasions.

Q. From whom?

(Testimony of Edward Park.)

A. I think one was from Mr. Bunker, he didn't want to sign his coal receipt, he said he was some coal short.

Q. And who else did you hear complain?

A. I think Mr. Hamilton thought he was short of coal, or said he was; but those were the only two. Take the "Korea," for instance, a steamer that we give 3,000 tons of coal to, sometimes 2800, Mr. Rossiter, the Chief Engineer, has told me repeatedly that he got his coal; he was taking 3,000 tons every time.

Q. When a round of tubs is weighed, is the coal generally taken from one particular part of the vessel or the barge, that is, when they weigh four tubs together?

A. There are two men working aft and two men working forward; they cannot take it from any particular place; two tubs are aft and two tubs are forward.

Q. They take the contents of two tubs from one particular part of the vessel and the contents of two tubs from another particular part of the vessel?

A. No, sir, the two tubs won't be together; there will be one on one side and one on the other; some of the tubs will get what they call wing-coal, a coarse coal.

Q. When you are working amidships how many tubs are in that particular part of the vessel?

A. There are always four tubs going unless it is very, very slow; then they will only run three.

Q. Mr. Park, if a tub is fillled to the waterline at the mouth [1362—1300] and filled, say, within a

(Testimony of Edward Park.)

couple of feet from the top at the bank, the tub only containing about three-quarters of its capacity, won't that tub tip by reason of the fact that the greater weight is at the mouth of the tub?

A. No, sir, they have got to be pretty full before they will dump; three-quarter tubs won't dump.

Q. Even if coal is filled to the waterline at the mouth, and is about 2 feet from the waterline at the back, you say it won't tip?

A. Oh, they keep their tubs even.

Q. I am assuming that they are kept even. Suppose we experiment with the tubs, as we did the other day, if a tub is filled to the waterline at the mouth and within 2 feet at the rear, won't that tub invariably tip on account of the superabundant weight at the front or mouth of the tub?

A. I have never seen a tub filled that way.

Mr. McCUTCHEN.—Do you claim that a tub could be filled that way?

Mr. SULLIVAN.—Yes, decidedly.

Mr. McCUTCHEN.—How would you hold the coal there?

Mr. ROCHE.—It will stay in the tub.

Mr. McCUTCHEN.—Yes, it will stay in the tub, but at that angle?

Mr. ROCHE.—Why, yes.

Mr. SULLIVAN.—Q. Wouldn't that tub fill that way?

A. I have never seen a tub filled that way. They have got to be full before they tip unless you break something or try to kill somebody down in the hold

(Testimony of Edward Park.)

or capsize the tub of coal into the hold.

Q. Do you know what would be the difference between a tub filled to the top with fine coal and a tub filled to the top with the [1363—1301] ordinary house coal—rough coal; say take one of these tubs of the "Comanche"?

A. You take a tub of fine coal and then take another tub of big lumps, lumps that they don't shovel in, but lumps that they put in by hand, large lumps, you will get from 180 lbs. or sometimes 200 lbs, difference between them; and then when a tub of lumps is piled up, the tub is not full, there are holes you could put your head in.

I have repeatedly seen the coal hosed down by employees of the Pacific Mail Steamship Company. That would occur when the coal is dry and would spoil the paint on the ship and interfere with the trimming. They generally use an ordinary ship's hose. They would play the water on the coal for quite a while until it got pretty wet to keep the dust down. I have called Mr. Chisholm's attention to that matter often, but it had to be continued for the reasons mentioned. I have repeatedly tried to get a reduction for the water from the Government, but the answer I always got was that the Government made no allowance for water. Mr. Chisholm knows that that is the situation.

Redirect Examination by Mr. KNIGHT.

The reason why we have to work at night on the "Korea" and "Siberia" is that after we have coaled a day or two on one side of those ships they take a

(Testimony of Edward Park.)

list to the other side and then we have to work at night to even the ship up; otherwise, we would be idle the next day. When I said a shovelful of coal would weigh 200 pounds, I was mistaken. My guess now is that it would weigh 20 or 25 pounds. During the last five or six years there have been 25 or 30 customhouse weighers engaged in weighing coal going into steamers belonging to the Pacific Mail Steamship Company.

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[1364—1302]

[Testimony of Fred Tietjen, for Defendants.]

FRED TIETJEN, a witness recalled for the defendants, testified as follows:

Direct Examination by Mr. KNIGHT.

Mr. KNIGHT.—Q. Mr. Tietjen, since you were on the stand here the other day, have you refreshed your memory as to the work which you performed on the Folsom Street scales for the Western Fuel Company?

A. I have, by looking over the records of the Fairbanks & Morse Company.

Q. Will you state whether or not, since 1905, you were engaged in any work on these scales for the Western Fuel Company?

A. I was. I have a copy of the different work that was done by Mr. Shaffer and myself at times. May 25, 1906—

Mr. SULLIVAN.—Q. (Intg.) Done by yourself,

(Testimony of Fred Tietjen.)

with Mr. Shaffer? A. With Mr. Shaffer.

Q. Or done by you at one time and Mr. Shaffer at another time?

A. Well, we generally worked together, and two of those jobs I certainly remember of doing alone.

Mr. KNIGHT.—Q. Will you state now, having refreshed your memory—you have a memorandum taken from the books of Fairbanks, Morse & Co.?

A. Yes.

Q. You took that memorandum off yourself, did you? A. I did.

Q. Will you state generally, Mr. Tietjen, what work was done on those scales of the Western Fuel Company since 1905?

A. I will; if you will allow me to read it.

Q. Yes.

A. May 25, 1906, time of mechanic patching levers in scale on south side of bunkers. Testing and adjusting scale. Time, Sunday, 1 day, 9 hours—Saturday and Monday nine hours.

Mr. ROCHE.—I understand, Mr. Knight, the witness is now referring to a time after the fire.

Mr. KNIGHT.—He testified that he did not do any work after [1365—1303] 1905.

Mr. ROCHE.—You say that he testified that he did not do any work after 1905?

Mr. KNIGHT.—That he did not work on these scales after 1905.

Mr. ROCHE.—The memorandum he is reading from shows work done in May, 1906.

(Testimony of Fred Tietjen.)

The WITNESS.—May, 1906, until the present time.

Mr. McCUTCHEN.—You brought out the fact that the Fairbanks, Morse Company had not done any work during that time, or anybody else.

Mr. SULLIVAN.—He testified as to September, 1905.

Mr. KNIGHT.—He said he did not do any work since the fire.

A. On July 7, 1906, sharpened and re-sealed the scale, that is, taking the scale out and repairing it.

Mr. SULLIVAN.—Q. Did you do that, yourself?

A. No; Mr. Shaffer. I probably worked with Mr. Shaffer at the time.

Mr. SULLIVAN.—We object to his testifying to anything that he, himself, did not do, we object to his testifying to any work Mr. Shaffer did, unless Mr. Shaffer worked with him.

The COURT.—Unless he knew Mr. Shaffer did it.

Mr. SULLIVAN.—The only way he can know is by being with him.

Mr. KNIGHT.—Q. Were you and Mr. Shaffer detailed for this character of work by Fairbanks, Morse & Company during these times?

A. Yes, we generally done mostly all the work together.

Q. Mr. Shaffer is the man whom you say is dead?

A. Yes. July 7, 1906, sharpened and re-sealed scale; that is, taking the scale out and overhauling it.

Q. You say "re-sealed"?

(Testimony of Fred Tietjen.)

A. Yes, that is the term we use, re-sealing the scale.

Q. Sharpened and re-sealed the scale?

A. Yes. [1366—1304]

Q. Will you state a little more specifically what that means, sealing the scales?

A. Sealing the scale is to adjust it and tighten up the bolts, and leave it according to what the United States weights call for—we call that sealing a scale.

Q. What was the date of the work?

A. July 7, 1906. We put new parts in the scale, two new knife edges, four bearing steels, 2 long lever nose irons. Those are parts put into the scale. On July 12, 1906, we tested the coal scales on bunkers on the south side of the bunkers.

Q. That is all on these Folsom Street bunkers?

A. Yes. On July 21, we took out south tramway scales, and brought them to the shop and repaired them taking 54 hours.

Mr. SULLIVAN.—Is that July 21st?

A. July 21st. In that scale were put four bearing steels, 2 long lever nose irons, and 2 steel yard-rod shackles. On September 8, 1906, wagon scale, sharpened—

Mr. SULLIVAN.—Of course, that is immaterial. That is a wagon scale.

Mr. KNIGHT.—Q. Is that the scale that is in front of the office on Stewart Street?

A. That is in front of the office.

Q. All we want, Mr. Tietjen, are the bunker scales.

A. Then we have September 20, 1906, on the Mis-

(Testimony of Fred Tietjen.)

sion Street bunker.

Q. September 20, 1906?

A. Time of mechanic adjusting and sealing, at Mission #2. On October 24, 1906, time of mechanic, one hour, Folsom Street wharf. October 27, 1906, time of mechanic sealing and testing and adjusting scales, four hours.

Mr. ROCHE.—Q. What scales is that?

A. That is the Folsom Street scales. December 1, 1906, tested scales at Folsom No. 2, north side, tramway, 2 hours. December 21, 1906, Folsom No. 2; one hinge for bunker track scale; repaired new truss rod, [1367—1305] fitted, and new parts fitted in scale and adjusted, 11 hours. February 5, 1907, 1 compound suspension scale repaired, cleaned, sharpened and re-sealed, 10 hours. That is for a barge scale.

Mr. KNIGHT.—Q. That was a platform scale on the barge? A. A suspension scale on a barge.

Mr. SULLIVAN.—That is immaterial.

A. January 30, Webster Street wharf. Time of mechanic testing scale, removing, patching and repairing, 2 days.

Q. We do not care for that.

A. February 14, 1907, barge "Ruth," Mission #2.

Q. You may confine yourself to the Folsom Street bunkers, if you will, the platform scales on the top of the bunkers.

A. Here is March 15, 1907, Folsom #2, 1—1166 scale repaired, new parts fitted—that is the number

(Testimony of Fred Tietjen.)

of the scale, the scale that is operated on Folsom Street.

Q. The scale they use on the barge? A. Yes.

Q. I do not want any barge scales, I only want the Folsom Street bunker scales.

A. April 24, 1907, Folsom #2 bunkers, time of mechanic testing scales with 1 ton of weights, 2½ hours.

Q. What date is that?

A. That is April 24, 1907. April 29, 1907, Folsom #2 bunkers, time mechanic testing scales, tested with 1 ton of weights and sealed, correct. July 5, 1907—that is Stewart Street, that is the same thing, that is a barge. September 30, that is another one. On March 11, 1908, tested scales at bunkers Folsom #2.

Q. Is that the scale on the bunkers, or on the barge?

A. On the bunkers, Folsom #2, tested scale on east and west side, 2 hours. July 24, 1908, examined scales at Folsom #2. **[1368—1306]** It had stopped weighing; Shaver early A. M. 24th. Examined two tramway scales, and adjusted. September 1, 1908, examined bunker scale, Stewart Street; repaired and adjusted as necessary. Shaver 3 P. M. testing bunker scales at Stewart Street; January 26, 1911, Folsom dock, sharpened scales. February 10, 1910, 1 nose iron, Folsom Street wharf. June 28, 1911, 1 hinge lever, 1 butt lever loop, 1 corner iron, 1 ten-inch corner iron.

Mr. ROCHE.—Q. Where was that?

A. That was Folsom Street. July 30, 1913, tested

(Testimony of Fred Tietjen.)

north side bunker scale, 10 pounds quick adjusted.

Mr. SULLIVAN.—That is 1913?

A. Yes. Tested north side bunker scale, 10 pounds quick adjusted, O. K. South side bunkers, levers were patched. That is the report that I have.

Cross-examination by Mr. SULLIVAN.

The memorandum which I have been using was made up by me in my own handwriting from the books of Fairbanks, Morse Company. The only items that I am perfectly certain refer to repairs that I made myself alone are two in number; July 21, 1906, and March 11, 1908. On the other items we worked together, probably. **[1369—1307]**

[Testimony of S. W. Parr, for Defendants.]

S. W. PARR, a witness called for the defendants and sworn, testified as follows:

Direct Examination by Mr. OLNEY.

I reside in Urbana, Illinois, and am now and have been for a little over 23 years Professor of Applied Chemistry in the University of Illinois. The duties of a professor of applied chemistry as distinguished from those of a professor of chemistry have to do with chemistry as it relates to practical and industrial affairs. I have made a special study of the subject of coal. I took up that study in 1896, 1897 or 1898, and most of my work in the professorship of applied chemistry has been in connection with coal since that time. The coal mining industry is important in Illinois, which state surpasses all other states in the Union in coal output except occasion-

(Testimony of S. W. Parr.)

ally West Virginia. As a rule, however, Illinois exceeds even West Virginia. In the course of my above-mentioned study of coal I have published twelve bulletins on the subject covering such topics as storage of coal, spontaneous combustion, firing of coal in operation, weathering of coal, deterioration, composition of coal ash, the compositions of Illinois coals and their properties (three bulletins), calometric heat value determination, etc. I have invented a number of devices for use by engineers primarily to give them trial of the property and character of coal. Those devices are in common use. I have the inspection of coal purchased for the State Institutions of Illinois. The duties of that coal inspection are to decide upon the value and properties of coal with a view to determining whether it should have penalties or premiums attached to it in settling with the coal operators, for if the coal has ingredients above a certain percentage **[1370—1308]** stipulated in the contracts of purchase it is my business to find out the same and report that to the board. The amount of coal purchased by Illinois for her state institutions ranges from 225,000 to 250,000 tons annually. In regard to my experience in a practical way in dealing with coal, I would say that I am sometimes called as a third party or umpire in settling disputes between operators and users. I think probably I have acted in that capacity half a dozen times in the last fifteen years. In this period I have made or supervised the examination of mines, and, under the direction of the State Geological Survey of Illi-

(Testimony of S. W. Parr.)

nois, have had prescribed the method of collecting the samples and deciding upon the quality and character of the coal furnished to the state. I have made or supervised something over 500 such examinations. I am a member of the American Society of Testing Materials. I am chairman of a committee of that society, namely: the Committee for Devising Standard Methods for Coal Inspection. These methods are standardized primarily for practical use, especially on the part of engineers, rather than for laboratory and scientific purposes.

BE IT REMEMBERED that thereupon the following testimony was given and that the following proceedings occurred:

Q. Has the study and investigation of coal, and particularly of the properties of coal and the ingredients found in coal, been a subject of general investigation through the United States in the last few years?

A. I do not quite catch your question.

Q. Are there other people besides yourself, for instance, other universities or government agencies, making **[1371—1309]** a study of the properties?

A. There are.

Mr. ROCHE.—That question is objected to upon the ground, may it please the Court, that it is immaterial, it certainly does not tend to throw any light upon the qualifications of the witness upon the stand.

Mr. OLNEY.—It is a preliminary question, and it has a bearing upon the question of information

(Testimony of S. W. Parr.)

which the defendants in this case might have in regard to inferences to be drawn from an overage.

Mr. ROCHE.—But the answer to that is, may it please the Court, of course, that the defendants in this case, who were acting in good faith, as they apparently claim they were, would have a right to testify on direct examination to what their information was upon these subjects.

The COURT.—The objection will be sustained. The fact that they might have obtained information, does not raise any presumption of innocence.

Mr. OLNEY.—I think, if the Court please, if as a matter of general investigation, and a matter of general information in the community, there is a presumption that they were informed in regard to these matters, it would not make any difference; it is a matter of general information and investigation.

The COURT.—What do you mean by general investigation? If it is so general that everybody should know it, then there is no occasion for him to testify at all on that subject.

Mr. OLNEY.—If your Honor please, everybody is not a coal man; I am holding this down now, to people engaged in the coal business; or, at any rate, to people **[1372—1310]** who are making a study of the subject.

The COURT.—Let him testify as to what he knows. If you have any other witnesses to testify to what they know, you can call them. The objection will be sustained.

Mr. OLNEY.—An exception.

(Testimony of S. W. Parr.)

Mr. ROCHE.—I ask that the answer be stricken out. I believe the witness answered before the objection was made.

The COURT.—Let it go out.

The investigations into the properties of coal which I have made have been concerned with changes in the weight of coal from time to time in the course of transportation, storage and marketing. I have made a study of coals generally in the United States which study has touched all coal-producing districts excepting a few small places in California or west of the Sierras. My study has covered all the fields east of the Sierras. As to foreign coals, I have worked with Welsh, English, Belgian and German coals, and I have some slight acquaintance with the coals of India. I have examined the Canadian coals east of the mountains and to a certain extent west of the mountains, and I have also examined Australian and Japanese coals. Coals are very subject to changes in weight in process of shipment, probably more so than any other commercial commodity. These changes are considerable in amount and take place within short intervals of time. There are two main causes for such changes in weight, one of which is oxidation and the other of which is changes in moisture content. That is true of all coals. **[1373—1311]**

BE IT REMEMBERED that thereupon the following testimony was given, and that the following proceedings occurred:

"Q. (Mr. OLNEY.) Is there any literature on

(Testimony of S. W. Parr.)

this subject, that is, have the results of this study into the characteristics of coal in this respect as to changes in weight been put in published form?

Mr. ROCHE.—Just a minute. That is objected to as immaterial, irrelevant and incompetent; and upon the further ground that if this evidence is sought to be introduced for the purpose of showing the familiarity of any of these defendants with these published reports, their familiarity must first be established.

The COURT.—Yes, I think so.

Mr. OLNEY.—We want to show something of the general condition of the art, and the general knowledge of the art in this respect. And regardless of whether the defendants themselves were acquainted with it, or not, we certainly have the right to show it, your Honor. It is introduced for that purpose; and for the purpose of showing that this witness' own conclusions are in accordance with the well-known investigation of other men upon that subject. Now, certainly all that goes to the weight and extent to which credibility is to be given to the witness' testimony, if it is material; it is something that is done every day in the courts. We have the right to prove the fact.

The COURT.—There is no question about proving the fact here.

Mr. OLNEY.—And I am showing not only that, your Honor, but also that this fact to which the witness is testifying [1374—1312] is a fact well known to the art, to the study upon this subject.

(Testimony of S. W. Parr.)

Certainly we ought to be able to prove that fact in connection with proving the statement of the witness himself.

The COURT.—It does not seem so to me. The result of this witness' investigations as a matter of fact as to whether coal will or will not change in weight is properly a matter for his testimony; but what other people may have ascertained by independent examination, in which he has had no part, does not seem to me to be material here. If this is a matter of common knowledge of which a court must take cognizance, then it is not a matter of testimony but a matter of information for the Court and the jury; but if it is not, then this witness can go no further than what he himself knows.

Mr. OLNEY.—Let me say this to your Honor, that it may well be that it is a matter of common knowledge by people who are acquainted with the subject, such that the Court could take judicial notice of; that even if the Court can taken Judicial notice of the matter, such as, for instance, the time on a certain day when the moon arose, still we claim that that is a matter that evidence can be introduced upon.

The COURT.—If the Court is going to determine it the Court must determine it upon its own investigation; it is not a matter that should be submitted to the varying judgment of twelve men.

Mr. OLNEY.—In many cases that sort of testimony is put before the jury.

The COURT.—That may be true and it may be put before the jury because nobody objects to it.

(Testimony of S. W. Parr.)

Mr. OLNEY.—And furthermore, if your Honor please, we would have the right in this case, I take it,—it was [1375—1313] practically admitted by counsel here the other day—to introduce scientific treatises on this subject.

Mr. SULLIVAN.—No, sir, nothing of the kind.

The COURT.—If there be such rule it is one that I have never heard of. But, of course, that is not saying that there is no such rule.

Mr. OLNEY.—I have authorities on the subject.

Mr. ROCHE.—The weight of authority is clearly the other way. On direct examination he has no right to introduce scientific works. He is called there for the purpose of giving evidence upon the very subject matter of these so called scientific works.

Mr. OLNEY.—On an examination of the rule, your Honor, you will find that the rule enunciated by Mr. Roche is practically confined to medical books, and they say that that is a science so uncertain as yet that they will not permit medical books to be introduced in evidence because of the uncertainty of opinions in that science. But that is not the case in connection with matters that have been definitely and finally ascertained by science. I think in that connection we have a right to go into the literature on the subject.

The COURT.—If that be true then there is no necessity of introducing a witness at all; all you have to do is to produce your books.

Mr. OLNEY.—You may have to do both.

(Testimony of S. W. Parr.)

The COURT.—I am not saying this in any spirit of levity at all, Mr. Olney, but if your contention be true, then there is no occasion to produce a witness at all; you can just produce your scientific books and introduce them.

Mr. OLNEY.—It may be that we will want to do both. **[1376—1314]**

The COURT.—If your contention be true you need not introduce the witness at all.

Mr. OLNEY.—That may be true. We may be entitled to simply rely on scientific works; we desire to do both things, as a matter of fact.

The COURT.—That may be true, but without some well-reasoned authority on the subject I am inclined to hold otherwise.

Mr. OLNEY.—This matter is taken up and discussed by Wigmore at considerable length. I am reading now from Vol. 3, Section 1690. The chapter is entitled, 'Exceptions to the hearsay rule.' Topic 9: 'Learned treatises.'

'This exception is usually spoken of as involving the use of "scientific books" or "medical books" or "books of science and art"; but the term "learned treatises" seems more accurate in indicating the scope of the doctrine. As an exception to the hearsay rule, it has obtained complete recognition in only one or two jurisdictions; but it deserves a fuller acceptance, and the precise bearings of the reasons for and against recognizing it deserve careful consideration.

(Testimony of S. W. Parr.)

'(1) More than one reason has been advanced for prohibiting the use of learned treatises in evidence; but the only legitimate one, and the one generally pointed out and relied upon in judicial opinion, is that such an offer of evidence purports to employ testimonially a statement made out of court by a person not subject to cross-examination; i. e., purports to violate the fundamental doctrine (*ante*, pp. 1362) of the Hearsay rule. That this is the main objection is indicated in the following passages':

And then follows the citation of a number of cases.

[1377—1315]

'Other reasons, however, which have occasionally been suggested, usually in connection with the preceding one, must be briefly noticed.

'(2) We are told that science is shifting; that experiment and discovery are continually altering scientific theories and rendering them valueless; so that a "medical book which was a standard last year becomes obsolete this year"; that there is no general agreement among scientists, and that testimony characterized by such instability and uncertainty is untrustworthy. Leaving aside for the moment the ignorant exaggeration in these changes, which attribute to the entire body of scientific knowledge the instability due to recent rapid progress in certain departments of the sciences, and ignore even in those departments the small proportion which the field of possible change bears to the large area of established truth, we find that the objection is in itself inconsistent with accepted legal practices, and would if

(Testimony of S. W. Parr.)

consistently applied exclude all testimony even on the stand from scientific witnesses. For if these works are rejected because they may not embody the latest results of science, what shall be said of specialist witnesses in general? Out of the hundreds of scientific experts who are this month testifying in courts of justice, how many are speaking from a thorough*ly* acquaintance with the latest researches in their subjects? For how many of them is it possible to maintain steady pace with the daily progress of science?' There is some more along that same line. And now I will read the following:

'(3) Another objection sometimes raised is the danger of confusing the jury by technical passages without **[1378—1316]** oral comment and simplification. A number of answers to this will suggest themselves; it is enough to point out that, so far as it is an appreciable danger, the counsel may be trusted to protect themselves, where necessary, against this danger by employing also an expert to take the stand.

'(4) Another objection, once made, is that the treatises may be used unfairly by taking passages which are explained away or contradicted in other books or in other parts of the book. Here, again, so far as the possibility is appreciable, the opposing counsel may be trusted to protect his client's interests, exactly as he does by bringing to the stand one expert to oppose another, and with much less difficulty and expense.

'All these objections, appearing in the beginning

(Testimony of S. W. Parr.)

as the casual thoughts of individual judges in past and less liberal generations, have been elevated to the rank of accepted reasons and given vogue by one or two writers on Evidence, and have thence found their way into many judicial opinions of the present generation. But for this, it is probable that the true reason for the rule of exclusion would, in this country at least, not have been obscured in the minds of a generation naturally so hostile to such illiberal notions.

'(5) There is also to be noticed, moreover, the original reason offered for exclusion by Chief Justice Tindal, in Collier v. Simpson, the starting point of the English decisions. "Physic," he said, when asked by counsel why he could not read to the jury a medical book as well as a law book, "depends more upon practice than law does"; meaning apparently that though the principles of law are chiefly obtained from books, the truths of medicine are to be sought **[1379—1317]** chiefly in the personal experience of physicians. It is almost needless to say that medical treatises cannot in these days be put on the shelf with the simple statement that medicine depends more on practice than the law does. The great shorehouses of medical experience are the books and journals of the profession. "Medical evidence," it has been truly said, altogether is little else than a reference to authority." The argument of Chief Justice Tindal has not reappeared.

'1. The Exception is Recognized. The grounds for recognizing the Exception, and its proper limi-

(Testimony of S. W. Parr.)

tations, if recognized, may be taken up in the light of the general considerations already mentioned for the other Hearsay Exceptions (*ante,* pps. 1421-1424).

'Sec. 1691. General Principle: (1) Necessity. The necessity (*ante,* p. 1421) seems palpable enough, if we examine carefully the results of the strict enforcement of the Hearsay rule. The ordinary expert witness in perhaps the larger proportion of the topics upon which he may be questioned, has not a knowledge derived from personal observations. He virtually reproduces, literally or in substance, conclusions of others which he accepts on the authority of the eminent names responsible for them. If, whenever this is discovered, we are to reject the evidence absolutely, then on all such matters the only resource is to search for a qualified expert, who may or may not be available within the jurisdiction. Even where such a person is legally procurable (all the chances being against it except in a few centres of population), the expense is frequently disproportionate. Costly litigation is the parasite **[1380—1318]** of justice; and we pay too high a price when we refuse to accept our information from a competent source ready at hand. Moreover, there are certain matters upon which the conclusions of two or three leaders in the scientific world are always preeminently desirable; and it is highly unsatisfactory that, except in the region where they may happen to live, the opinions of world-famous investigators should have no standing of their own. Whether

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such persons are legally unavailable, or whether it is merely a question of relative expense, the principle of necessity (*ante,* p. 1421) is equally satisfied; and we should be permitted to avail ourselves of their testimony in the printed form in which it is most convenient.

'The proper rule would be for the Court to allow the use of a printed treatise, unless in its discretion, considering all the circumstances, the author if available should be summoned. In practice, the Courts which allow the use of learned treatises apparently do not impose any such condition.

'Sec. 1692. Same: (2) Trustworthiness. Under the second general consideration for Hearsay exceptions (*ante,* p. 1422) the question here is whether there are any circumstances attending the publication of a learned treatise which give a fair guarantee of trustworthiness.'

These reasons are so cogent, your Honor, that I wish to impress them upon you.

The COURT.—I know they are so cogent, but they may well have been presented to a legislature which has the power to change rules of evidence.

Mr. OLNEY.—I want to call your Honor's attention to an authority in that connection, Western Assurance Company vs. Mohlman, 83 Federal, 811, a decision by Judge Lacombe **[1381—1319]** in the Circuit Court of Appeals. The case was before Peckham, Circuit Judge and Lacombe and Shipman, Circuit Judges.

(Testimony of S. W. Parr.)

'The next group of assignments of error raises the question as to the propriety of allowing one of the witnesses, a civil engineer, and expert in heavy construction work, to read excerpts from scientific books when giving his testimony. The general proposition that scientific books are not to be read in evidence is a familiar one, and many citations from text writers and reported cases are found in the brief of the plaintiff in error. Nearly all the reported cases deal with medical works, and most excellent reasons for the application of the general rule in such cases may be found therein. But the rule is not of universal application. It would be a reproach to the administration of the law if it were so. Records of observations are undoubtedly secondary evidence, but, if all such records were excluded from the sources of knowledge available to a court of justice, it would frequently find itself unable to obtain information which was open to every individual in the community. It has been held repeatedly that standard life and annuity tables, showing at any age the probable duration of life, are competent evidence (Railroad Co. v. Putnam, 118 U. S. 554, 7 Sup. Ct. 1); and yet these tables show merely the deductions from records of past transactions when neither the record of the transactions nor the individual who has worked out the deductions is called to testify to the accuracy of his work, or to the conditions under which it was performed. So, too, almanacs, astronomical calculations, tables of logarithms, interest tables, weather reports, tables of the rise and fall of the tide, have

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been admitted in evidence.' [1382—1320]

Mr. ROCHE.—Mr. Olney, is it not a fact that in each instance there the Court will take judicial notice of those facts, including the mortality tables? The Supreme Court of this State and the Supreme Court of the United States have so decided.

Mr. OLNEY.—It does not make any difference whether that is so, or not. If Mr. Roche's proposition is true, and the Court will take judicial notice of it, then the Court will take judicial notice of what we are going to put in evidence here, and for the purpose of getting it before the Court and the jury, we are entitled to put it in evidence. That is exactly what was done in this particular case, where this witness read excerpts from scientific works on the subject upon which he was testifying.

Mr. McCUTCHEN.—And that was for the purpose of showing the resistance of certain material, or the tensile strength of certain material, was it not?

Mr. OLNEY.—Yes.

'In an opinion approving of the admission of market reports, upon which the commercial world relies, is found the following pertinent suggestion of Judge Cooley:

' "As a matter of fact, such reports, which are based upon a general survey of the whole market, and are constantly received and acted upon by dealers, are far more satisfactory and reliable than individual entries, or individual sales or inquiries; and courts would justly be the subject of ridicule if they should deliberately shut their eyes to the sources

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of information which the rest of the world relies upon, and demand evidence of a less certain and satisfactory character." Sisson v. Railroad Co., 14 Mich. 497. **[1383—1321]**

'The particular excerpts complained of in the case at bar are these: Certain reports of the United States department of agriculture, prepared under the direction of the chief of the division of forestry, contain tables which comprise the results of over 2,000 tests by the United States Government of the crushing strength of different kinds of timber, prepared expressly to increase the knowledge of timbers grown in this country for the benefit of merchants and dealers and builders and engineers. The report is a recognized authority in the engineer's profession. From the tables the witness read the "results of investigation on long leaf pine," which was the kind of timber in the posts the cause of whose giving way was the subject of dispute. The next book produced was Kent's Mechanical Engineer's Pocketbook,—the last edition of 1896—which, it is not disputed, is a recognized authority. "Every mechanical engineer," says the witness, "has it on his shelf." From a table in this book, giving the crushing strength of timber, the witness read a statement of such strength, per square inch, of the kind of pine of which the posts were made. The third book is Johnson's Strains in Frame Structures, also concededly a recognized authority. It contained similar tables, and a similar excerpt was read. That information of great value is obtained by multiplying such tests and tabulating

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the results is surely self-evidence. Under the rule contended for, the results are surely self-evident. Under the rule contended for that valuable information would be available for the use of a court of justice so long as the men who made the tests and prepared the tabulations were living and producible, but after their death or disappearance the information they had gathered would be lost to the Court, although available for **[1384—1322]** every one else in the community, and relied upon by engineers and builders whenever a new structure is in process of erection. Upon the precise point here presented the diligence of counsel has not succeeded in discovering a single authority. We fell, therefore, no hesitancy in so modifying the general rule as to *feel, therefore, no hesitancy in so modifying the general rule as to* hold that, where the scientific work containing them is concededly recognized as a standard authority by the profession, statistics of mechanical experiments and tabulations of the results thereof may be read in evidence by an expert witness in support of his professional opinion, when such statistics and tabulations are generally relied upon by experts in the particular field of the mechanic arts with which such statistics and tabulations are concerned.'

We have in this State Section 1936 of the Code of Civil Procedure, which reads:

'Historical works, books of science or art, and published maps or charts, when made by persons indif-

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ferent between the parties are *prima facie* evidence of facts of general notoriety and interest.'

And so I say to your Honor that the legislature has acted on this particular thing.

The COURT.—If this comes within that class, yes, of general notoriety and interest.

Mr. OLNEY.—Books of science and art are *prima facie* evidence of facts of general notoriety and interest.

The COURT.—Those are matters that the Court will take judicial knowledge of, of course.

Mr. ROCHE.—Yes, your Honor.

Mr. OLNEY.—It is more than that. This says, 'they [**1385—1323**] are admissible in evidence and they are *prima facie* evidence' of these matters; what I was going to say to your Honor is this: That if that section is limited to facts of general notoriety and interest, meaning thereby matters that are known to people in the community there would be no use of the section at all because there would be no reason for the introduction of the books of science or art or historical works under those circumstances. To give it any force at all the section must be designed to cover those cases where there is in the profession or in the particular study matter of general notoriety and interest, matter on which there is a practical agreement of opinion by the people who are studying it and are concerned in it, if this section is to be given any force. It must appear that that is the subject and the instance in which works of science or art and historical works and published maps and things of

(Testimony of S. W. Parr.)

that sort can be introduced. That is exactly the case here.

The COURT.—It does not seem so to me. You have produced a witness here who has, according to his own testimony, made very thorough experiments into these matters; he may testify to the results ascertained; but beyond that, I don't think he is competent to testify. If the matter is of such general notoriety as to warrant the admission of it in evidence we would probably know something of it ourselves without being compelled to resort to this witness to testify that these experiments have been made by others.

Mr. OLNEY.—I don't want to encroach upon your Honor's patience in this matter, but I say that your Honor's remark there indicates that the section of the Code of Civil Procedure which I read to your Honor was designed to cover **[1386—1324]** another case entirely, because there would be no reason for that section if the matter of public notoriety and interest which is spoken of there was a matter which everybody knew about anyhow.

Mr. ROCHE.—Mr. Olney, what difference would there be between a case such as this where you are trying to establish some scientific work on coal and a scientific work on surgery, which so far as our knowledge goes, is claimed now to be an exact science. The Code does not distinguish between a case such as this and any other case. It is true that medicine is not an exact science and yet it is a science, and the statute to which Mr. Olney has directed your Honor's

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attention does not recognize any distinction between what counsel calls an inexact science and an exact science. If this is true, then any book on surgery or any book on science would be equally admissible.

Mr. OLNEY.—I would say that the answer to that, Mr. Roche, is that these rulings of the courts excluding medical books and books on surgery were made years ago and have been followed blindly by the Courts since; and that in the last 20 years—yes, in the last 10 years there has been such progress in the art of medicine and surgery that it is almost a disgrace to the courts that they shut out these reliable sources of information from themselves by blindly following the old rule.

The COURT.—I understand that, but if we can each make a rule for ourselves we would then have no rule at all. It would be a very different matter, of course—if this evidence is admissible, the Government has no relief at all; if it were offered by the Government and admitted over objection **[1387—1325]** by the defendants, and admitted improperly, the defendants would have relief; but the Government is in a position where if the Court errs against it, it can have no relief whatsoever.

Mr. OLNEY.—If they have any works of science they can produce them.

The COURT.—I am speaking of the application of the rule. The objection is sustained.

Mr. OLNEY.—We take an exception."

The Bureau of Mines of the United States Government has made a study of this subject of coal and be-

(Testimony of S. W. Parr.)

fore the Bureau of Mines was organized the United States Geological Survey made a similar study. The men in connection with the United States Bureau of Mines who are particularly engaged in this subject are Porter, Fieldner and Davis. They are still in the service of the Government. I am acquainted with them.

"Q. Where are they located?

Mr. SULLIVAN.—We object to that as immaterial.

The COURT.—What is the materiality of that, Mr. Olney?

Mr. SULLIVAN.—Do you want to know their opinions through this witness?

Mr. OLNEY.—No, not through this witness.

The COURT.—What is the materiality of this line?

Mr. OLNEY.—I wanted to bring home some Government publications, if the Court please, in connection with the Bureau of Mines.

The COURT.—If the Government publications are admissible in evidence they are admissible for that reason, **[1388—1326]** and they do not need any support.

Mr. OLNEY.—Very well, your Honor.

Mr. McCUTCHEN.—Will your Honor permit a suggestion now?

The COURT.—Yes.

Mr. McCUTCHEN.—I have an impression that Mr. Olney wants to develop the fact that these gentlemen are in charge of stations maintained by the

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Government for the purpose of testing coals and determining the changes in weights, among other things, that coal undergoes. If the witness is familiar with that, it seems to me that he is competent to prove it, and in connection with any offer of Government publications audited by these three men.

The COURT.—I don't think that adds anything to it. They are either admissible and carry their own authenticity or confirmation with them, or they are not; what you are trying to do is to support by the testimony of this witness the testimony of certain other witnesses who are going to be introduced by having him testify that they are real experts.

Mr. McCUTCHEN.—I think your Honor misapprehends the matter.

The COURT.—In its last analysis I think it is that.

Mr. OLNEY.—It is just the other way, your Honor. We want to support the testimony of this witness—

The COURT.—Well, yes, if you called them to the stand, they couldn't tell you that this witness is a thorough expert. Now, you are trying to have him say that they are thorough experts. **[1389—1327]**

Mr. OLNEY.—No, that is not it, your Honor.

The COURT.—Then I can't follow you.

Mr. OLNEY.—We have called this witness and we are going to ask him certain questions which will deal specifically with the particular subject that is under investigation in this trial.

The COURT.—Yes, that is very proper if we ever get that far.

(Testimony of S. W. Parr.)

Mr. OLNEY.—In addition to that, your Honor, for the purpose of corroborating the testimony of this witness, so to speak, we want to show that his testimony is in accord with the general scientific opinion upon this subject.

The COURT.—We have passed that, and we have come to certain Governmental reports.

Mr. OLNEY.—I desire to do the same thing with this.

The COURT.—They may be admissible upon another ground, but not because this witness says the man who produces them is an expert, and a good one, or because they have been located at any certain place. If they are admissible as Government reports, they are admissible under the rule that permits them to be introduced.

Mr. McCUTCHEN.—Might it not add to the value of these publications?

The COURT.—It might, but should it?

Mr. McCUTCHEN.—I think it should.

The COURT.—I don't think so; they either stand or fall by themselves.

Mr. McCUTCHEN.—If it is shown that the authors of these reports were men actually in charge of Governmental stations that were organized and conducted for the purpose of [1390—1328] dealing with the subject with which the reports deal, I think that would be proper.

The COURT.—I suppose the Government would not put them out unless they were proper. They put them out because they are reports which came into

(Testimony of S. W. Parr.)

them in due course. If they were sent in by some fellow who was not so expert, I suppose the Government would print and publish them just the same. The objection is sustained."

BE IT REMEMBERED that thereupon the following testimony was given and that the following proceedings occurred:

"Mr. OLNEY.—Q. Does the United States Bureau of Mines in addition to making what I may call a scientific or theoretical study of coal perform as a part of its duties any function in connection with the practical application of its studies and inspection?

Mr. ROCHE.—That is objected to as calling for the conclusion of the witness. It asks the witness to state what these men do.

The COURT.—The objection is overruled.

A. The Department does carry on such work.

Mr. OLNEY.—Q. And what is the nature of that work?

A. It is in the nature of coal inspection covering the purchases made by the Government.

Q. Just state a little more fully what the nature of that inspection is?

Mr. SULLIVAN.—We object to that as calling for a legal conclusion. The law defines the duties of those officials.

Mr. OLNEY.—I am not asking what their duty is; **[1391—1329]** I am asking him what they do.

Mr. SULLIVAN.—Well, I suppose they do their duty; whatever their duty is, they do; you are asking the witness to express a conclusion of law.

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The COURT.—The objection is overruled.

A. The Government purchases between 5,000,000 and $6,000,000 worth of coal annually, and it is the duty of these men to see that the conditions under which the coal is contracted for are met by taking samples of the coal and determining whether it is according to the standards of the specifications.

The COURT.—Q. Then their duties relate and cover really much the same as yours do, except on a larger scale, much the same as your duties in relation to the State of Illinois?

A. Precisely so.

Mr. OLNEY.—Q. You say this coal purchased by the Government, was purchased according to specifications, and that it is the duty of the Bureau of Mines to see that the coal comes up to the specifications of the contract.

Mr. ROCHE.—That question is objected to, may it please the Court, upon the ground already stated. Counsel is endeavoring to qualify now some of these Government officials through the testimony of the witness upon the stand.

Mr. OLNEY.—No, I am not, and I have not finished the question. I will finish it.

Q. I am going to ask you, Professor Parr, if these specifications in the Government contracts for the purchase of coal contain any specifications as to moisture content?

Mr. SULLIVAN.—We object to that as calling for [1392—1330] hearsay evidence.

The COURT.—The objection is sustained.

(Testimony of S. W. Parr.)

Mr. STANLEY MOORE.—We take an exception.

Mr. OLNEY.—Q. Do you know, of your own personal knowledge, what these Government contracts call for by way of specifications as to moisture content?

Mr. SULLIVAN.—We object to that as calling for incompetent testimony. The specifications speak for themselves.

The COURT.—The objection is sustained.

Mr. OLNEY.—We note an exception.

Q. What is the practice pursued by large purchasers of coal in the east with reference to the purchase of coal and providing as to moisture content?

Mr. ROCHE.—We object to the question as immaterial, irrelevant and incompetent, and we have nothing at all to do with what practice is now being or has been heretofore pursued in the east or by any corporation or by any firm or person other than the Western Fuel Company.

The COURT.—The objection is sustained.

Mr. OLNEY.—We note an exception."

I have personal knowledge that as a matter of fact the analyses and samples which are taken by the Bureau of Mines on the purchase of coal by the Government are concerned with the determination of the moisture content of coal. The fact is that the moisture contents is a very important item, and is always considered. Good practice would prescribe that in sampling coal for the purpose of determining whether deliveries conform to the specifications of the **[1393**

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—1331] contract one sample should be taken in each car lot or 50 tons. Samples should be taken thus often because of the variation in moisture content from car to car or even sometimes in different parts of the same car. These variations in moisture content are almost identical with the changes in the weight of the coal. Furthermore, these variotions in moistrue content and in weight are not confined to differences between different coals or coals from different mines, but are found in the same coal from the same mine equally as frequently as between different coals. The moisture content of coal from any mine or vein is not variable, but is fairly constant as the coal is mined from year to year,—that is to say, is substantiallly the same at the point of taking out from the mine. The variations in coal from different parts of the mine are small and are confiend within narrow limits.

BE IT REMEMBERED that thereupon the following testimony was given and that the following proceedings occurred:

Mr. OLNEY.—Q. Then, as I get you, the observed facts upon this point are that the moisture content of coal from any particular part of the mine is fairly constant as mined. A. It is.

Q. But after this time the moisture content is variable? A. It is.

Q. And these variations in the moisture content take place in the course of shipment and in storage and marketing? A. They do.

Q. These variations, as I understand you, corre-

(Testimony of S. W. Parr.)

spond to variations in weight.

A. They do. [1394—1332]

Q. Within what range are these variations in weight observed? A. Under shipping conditions?

Q. Yes, under practical conditions, within what range would these variations in weight take place?

A. Between a range of from one to 15 per cent.

Q. One to 15 per cent of the weight of the coal?

A. Of the weight of the coal.

Q. Is that 15 per cent a percentage found in actual commercial practice? A. It is.

Q. Is this change in weight recognized commercially? A. It is.

Q. Can you give an instance of that?

Mr. ROCHE.—One minute. That is objected to, may it please the Court, as calling for the opinion of the witness and as being something which is not the subject of expert evidence.

The COURT.—The objection is sustained.

Mr. OLNEY.—It is not a matter of opinion; I am asking him to give an instance where it is recognized commercially.

The COURT.—Even that is not a material matter here.

Mr. OLNEY.—We note an exception.

The United States Geological Survey and the Bureau of Mines have issued publications in which this matter of variation in the weight of coal due to variation in moisture content is touched upon. (Bulletin Number 41 of the Department of the Interior, Bureau of Mines was here introduced in evidence as

(Testimony of S. W. Parr.)

Defendants' Exhibit "GG." The witness was here [1395—1333] asked to call to the attention of the jury, by reference to tables beginning on page 37 of said bulletin, which tables record the results of the analyses of coals delivered to the Government under contracts in 1909 and 1910, those cases where on the same coals the moisture content from time to time has varied by considerable percentages, the witness having first testified that he was acquainted with the practice of the Bureau of Mines in taking the samples for said tables and having explained that the samples were taken at the point of delivery to the Government, and that the tables show in each case where the coal comes from, that is to say, the source of the coal, and having testified that all of the coal in each particular table comes from the same source.)

(To Mr. ROCHE.) The table does not show how long before the examination the coal was taken from the mine. The Government is not concerned with the time element.

Mr. OLNEY.—Q. Professor Parr, will you take the cases of the large differences?

A. Here is a case of 2.4 per cent at one time and 6.3% at another time; that lacks very little of being 4%.

The COURT.—Q. 4%, and that shows that coal under certain conditions will vary 4%; that is all it can show, isn't it?

A. Yes. Here is another case of 1.5 at one time and another time 4.5; that is about 2½% difference. Here is a case of 2% and 4.8%, a difference of 2.8%.

(Testimony of S. W. Parr.)

Here is a case of 2.39%, and the same coal 9.9%; that is a difference of 6½ per cent. Page 43 was the last item. The next page is 50; that is a case of 4.1%, and at another time 11.25%, a difference of 7.2%. Here is a case of 2.4%, and at another time 7.95, a difference of 5.7%. Here is a case of 1.3, and the same coal at another time at 5.8, which [**1396—1334**] is a difference of 4½%. Here is one of 1.9, and 6.35 at another; that is about 4½% difference. Those are all the cases of moisture extremes.

Mr. OLNEY.—Q. There are other tables there in which the difference in moisture content is not so great, are there not?

A. There are.

Q. As between those tables in which the variation is great and those in which the variation is small, which are the significant as indicating the susceptibility of coal in the changing of weight?

A. Those showing extremes are the more significant because they show what external conditions may do and we do not know in the other cases the condition the coal was subjected to.

Mr. OLNEY.—Q. Is there any difference between the moisture content in different seasons in these tables? A. There is.

Q. What is that difference in general?

A. In that part of the year when the rainfall is occurring it seems to be quite consistent that there is an increase in the moisture content.

Q. That is, according to the tables?

A. According to the tables.

(Testimony of S. W. Parr.)

Q. By the way, in this connection, Professor, let me ask you are you acquainted with the method of analyses pursued by the Government in getting these results? A. I am.

Q. Is that method such that any error occurring in the course of it will give too little or too great moisture, or will it average itself in the long run? [1397—1335]

A. It is of such character that the differences are under rather than over, and the moisture content as indicated is small rather than great; I mean to say the analysis of an individual coal will tend to give a too low moisture content.

Q. In other words, these variations of which you speak are in all probability—not in all probability—I will change that. In other words, the method of the Government analysis is such that the actual differences in the moisture content are greater than would be indicated by these published results?

A. That is true.

The COURT.—Q. Would that be true wholly, Professor? When they start in with their first analysis don't they take the *little* under too?

A. The conditions of the analyses are such in the high moisture coals that it is very difficult to avoid losing more water relatively than in the low moisture coals.

Mr. OLNEY.—Q. Will you just explain that fully to the Court and the jury, that pursuing the same method of analysis on a coal with a low moisture content as on a coal with a high moisture content

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you nevertheless, if there is any error, will find the error in favor of a larger variation between the two than is actual; that is, why is not the percentage of error the same in both character of analyses, both where the percentage of moisture content is low and where it is high?

A. In that sample which at the start has low moisture content, the tendency of that moisture to get away is rather slight; that is to say, it is already dry and the air will not dry it any more. When working with a substance with much water in it, it requires exceedingly great care to avoid loss of **[1398—1336]** moisture which does not come into the account; it gets away without being weighed into the final estimation; hence a high moisture coal is very certain to have more moisture in it than the analytical results will really show at the end. This is one of the most difficult things that the chemist has to deal with.

I have made myself familiar with the character of Wellington coal in British Columbia and of Richmond coal from Australia and of Japanese coal. All these coals are of the bituminous type. I have made analyses of these classes of coal to determine their moisture content. They are all of the low moisture class. Bituminous coals will vary in moisture content, and in weight from 2% for the low moisture type to 14 or 15% for the high moisture type. These particular coals have a moisture content varying from 3 to possibly 3½%. The time at which I speak of them as low moisture coals is the time of mining,

(Testimony of S. W. Parr.)

that is of the breaking of the coal out from the seam at the face of the vein. There is no difference in the behavior of low moisture coals on the one hand and of high moisture coals on the other hand after they have been taken from the mines and subjected to the air and the weather, excepting in the amount of of change. They both change alike but different in degree.

Q. Will you take, for instance, a high moisture coal, a coal with a high moisture content when mined and explain what would take place with it, what change would take place in its moisture content as compared with a low moisture coal and the changes which take place in its moisture content in the ordinary process of handling?

A. In the case of a high moisture coal, that is, one with 12 to 15 per cent of moisture, the tendency **[1399—1337]** with that coal is to lose moisture, and the quantity in process of shipment where it is not exposed to weather conditions would be greater than in the case of a low moisture coal where it started out with 3 per cent of moisture and was not exposed to weather conditions, the loss of moisture in the case of that coal would be smaller in amount.

Mr. SULLIVAN.—Q. Proportionately?

A. Proportionately smaller.

Mr. OLNEY.—Q. That is, you are speaking now of a high moisture coal which is transported under cover and a high moisture coal which is not transported under cover; is that correct?

A. If both coals are transported under cover the

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tendency will be on the part of both to lose moisture; the high moisture coal will lose more; it may lose 2% of moisture. The low moisture coal in transporting under cover cannot lose proportionately the same amount even; it may lose as much as ½% possibly in the process of transportation.

Q. Under the same circumstances what will happen with the high moisture coal?

A. If it is transported under cover and without access to moisture?

Q. Yes. A. It will lose possibly 2 per cent.

Q. What I really had in mind, Professor Parr, was this: Suppose you take a low moisture coal and also a high moisture coal, and you either ship them or store them, open to the weather, and subject to the weather, how will those coals behave with relation to their moisture content and changes in weight compared one with the other?

A. If they are exposed to the weather there can be nothing happen really to the low moisture coal excepting to [1400—1338] increase in weight, because it is down at the minimum point to start with. The high moisture coal normally will increase in weight, but its ability to lose may offset that so that the ultimate result may be about even. But if it is exposed to the weather, to certain conditions of weather, of course the high moisture coal will run beyond its normal moisture and increase in weight proportionally.

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Mr. OLNEY.—Q. Take this case: Take the case of

(Testimony of S. W. Parr.)
coal which is mined at Nanaimo, and almost immediately put on board ship there for San Francisco, and brought to San Francisco by ship, under hatches, that is, under deck, and not exposed to the weather. Now, what changes in moisture or content or in weight will occur in that coal from the time it is mined at Nanaimo until it arrives at San Francisco and is weighed here?

A. It will lose in weight while it is left uncovered to a certain amount, but as I stated before, that amount of loss will be very small. It is the tendency of all coals, upon being broken out from the mine, to lose the moisture which they have entrapped or entrained in the texture of the coal, being under great pressure; when that pressure is removed, all coals lose a small amount of moisture which they have inherent in the coal, and this would be the characteristic of the coal from Nanaimo, as well as any other coal.

Q. You say the moisture being in the coal under great pressure, you refer to the coal in the seam, do you not? A. In the seam.

Q. Suppose the cargo of coal that was loaded at Nanaimo was wet when loaded, whether from rain or from any other cause, would that make any difference?

A. It would **[1401—1339]** be a little higher moisture coal, and would have a higher chance for loss, the loss would be greater—could be greater than if it started out with a lower moisture content.

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Q. Take the case of coal arriving here from

(Testimony of S. W. Parr.)

Nanaimo, Professor Parr, and being stored in the yard, say, or in the bunkers of the Western Fuel Company, will that coal lose weight or gain weight, or is there any general rule as to its gaining weight or losing weight during its course of storage there?

A. Considering the character of the coal, it could not lose weight; it would gain weight if it did anything.

Q. To what extent, and dependent upon what conditions would it gain weight?

A. It would depend on the climatic conditions, and the extent would be within the range, I should say, of eight, nine or ten per cent.

The COURT.—Q. You are speaking of the weight, now, Professor? A. Of the weight.

Mr. SULLIVAN.—I understand that is the amount gained.

Mr. McCUTCHEN.—The witness says it would be within that range.

Mr. SULLIVAN.—Yes, 10 per cent.

Mr. OLNEY.—Q. You mean by that that it would gain eight or nine per cent in moisture content, and accordingly in weight; is that correct?

A. Yes, sir.

The COURT.—Q. Do I understand that if it gains 8 per cent in moisture content, it gains 8 per cent in weight?

A. It does; the two are synonymous practically. [1402—1340]

Q. You measure the moisture content by weight?

A. By weight.

(Testimony of S. W. Parr.)

Mr. OLNEY.—Q. These percentages of moisture content are really percentages of weight, are they not? A. They are percentages of weight.

In case of coal being wet, there would be a great difference between the proportion to which fine coal and lump coal respectively would increase in weight. The rule in that respect is that the capacity of the coal to hold water would be directly in proportion to the amount of fine coal. Lump coal without any fines in it would perhaps have its upper limit of moisture content at 2 or 3 per cent. The finer the coal the more moisture it would hold up to 8 or 10 per cent, in the general type of coal that is handled here. Some of the coals have so much fine material in them that a moisture content might run up as high as 12 or 15 per cent; that would be entirely reasonable. As to the coals which I have observed here in which the moisture content might run that high or in which that increase in weight might have occurred, I would say that the Japanese coals have a larger amount of fines and consequently would have the higher weight. I have observed vessels discharging here and my statement is based upon my observations in that connection. I have seen a number of Australian cargoes discharged here. They do not have as much fines and would not hold as much water as the Japanese coals. A pile of fine coal once wet drys out only superficially—I should say to the extent of 5 or 6 inches under the surface—deeper down it would be wet still if it were wet originally. The reason for that is that the drying out of any material is depend-

(Testimony of S. W. Parr.)

ent upon the circulation of air and if fine coal is left undisturbed and is wet it cannot dry out very far below the surface. In the **[1403—1341]** case of a pile of fine coal 8 or 10 or 20 feet deep, for instance, which was built up during the rainy weather in the spring and allowed to remain all summer exposed to the open air without any rain whatever upon it, the loss of moisture during the whole summer would be very slight and would be confined exclusively to the surface of the pile 5 or 6 inches deep. In the case of a pile of what might be called average coal, that is, coal made up of lumps and fines as I have seen it discharged from these steamers, there would be more drying out because the air could circulate among the lumps, but a little below the surface the fines settle and accumulate and stop the circulation of air so that the loss of moisture in the pile would be slightly affected.

I know of an experiment or test being made to ascertain the percentage of screenings in a lot of coal received by steamer here. Certain bunkers were filled with Australian coal as it was discharged from the importing steamer and the weight taken. The scales of the lumps from those bunkers were retained and kept separately so that a percentage could be calculated as to the fines that resulted. The percentage found was a trifle over 24%. I think several hundred tons were observed in the course of that test. This lot of coal would be a very fair average, in respect of percentage of screenings or fine coal, of the Australian coals generally as I have seen them

(Testimony of S. W. Parr.)

coming into this port, but would not be a fair representation of the Wellington coals, though it would be as to them an approximation.

I have made tests to verify, with regard to the particular coal involved here, namely, the British Columbia coal, the Australian coal and the Japanese coal, those general propositions to which I have testified. Thus I have twice visited **[1404—1342]** the mines of the Western Fuel Company at Nanaimo; in August and December, respectively, of last year. On the August visit I made a comparison between coals that had been under cover and coals that had been out in the rain.

Q. Just state to the jury what the results of that comparison were.

A. In the case of coal which had been in storage, under cover, the moisture had not varied greatly from the moisture in the vein sample; it had dropped down about half a per cent, so that the loss in the weight of the car sample under cover would be represented by a half per cent less than when it was put in the storage. In the case of a car out in the open, the moisture content was 4.4 per cent, showing an increase of substantially 2 per cent over the car under cover; so that the increase in weight in the car in the open was substantially 2 per cent.

The test was made between August 25 and September 1, last. The coal had been out in the weather presumably 4 or 5 weeks. The rainfall, according to the weather report in that period had been a trifle under one inch. The particular car from which I

(Testimony of S. W. Parr.)

took my sample had been rained upon not over 2 or three weeks before the sample was taken. During that 2 or 3 weeks there had been no rain. I did not take a sample of the coal from the top of the car which had been rained upon, but my companion, Professor Sommermeier, did. The surface of the coal on top of the car in the open was dry down 6 or 8 inches. It was an open gondola car, not of steel. The coal was largely fines and its depth in the car approximately 5 feet. **[1405—1343]**

I took some samples of the Australian coal here in San Francisco before and after the rain. A sample of coal was taken on December 10, 1913, from the steamship "Lord Sefton." As the coal was being unloaded the percentage of moisture content was 3.11. The result of a rainfall on the night of December 10th was .2% of an inch. The moisture in the coal for a distance of 10 or 12 inches below the surface on the next morning after the rain was 12.15%, showing an increase in weight for that part of the coal of 9% to the depth to which the water had penetrated. In the meantime, and while this coal had been exposed to the rain, it was in a bunker on the discharge or land side of the Folsom Street bunkers. That is to say, it was in one of the regular bunkers there. I also made a test as to the capacity of fines, as they come from the screens, to hold water. A hundred pound lot of coal was saturated to the point where it would not hold any more water and given an hour to retain it to see what its capacity

(Testimony of S. W. Parr.)
for moisture was. It showed an increase in weight of 9.25%.

Q. Now, Professor, with relation to determining the proportion which lump coal, for instance, will retain as compared with the proportion which fine coal will retain, did you make any test upon that subject? A. Not on this specific coal.

Q. Did you make it in regard to other lots of coal, Nanaimo coal and Australian coal?

A. I conducted experiments on smaller lots of all three types of coals to determine the total capacity of fines, and the coal from which the fines had been taken. I misunderstood you. They were not large lumps, they were small, with all the fine material out. I made such experiment. **[1406—1344]**

Q. Did you make any tests to determine what proportion *would* the coal consisting of fines, or having fines in it, would take of water as compared with coals from which the fines had been removed?

A. Yes, I did.

Q. Will you state the results of those tests, and what the tests were?

A. A coal which is fine enough to and will pass a ten-mesh sieve, that is, a sieve with ten divisions to the inch, has a capacity—these particular coals, and in this they do not differ from other coals, but these coals show a capacity for water of 36 to 42 per cent in the extremes. The same coals with all the fines taken out, but still being fine enough to pass through a one-quarter inch mesh sieve, that is, with four divisions to the inch, have a capacity for water vary-

(Testimony of S. W. Parr.)

ing from 3–1/3 per cent to 8.19 per cent in the extreme.

I have testified that there are two causes for variation in the weight of coal—one the moisture content and the other oxidation. Oxidation always increases the weight of coal. If there is oxidation there is an increase in weight so far as it acts.

Q. Will you explain to the jury just what that process (of oxidation) is, and what goes on?

A. There are two actions due to the oxygen of the air; one is an oxidation of the carbonaceous matter of the coal, the other is an oxidation of the sulphur in the coal, which is present in the form of yellow flakes, or small particles which the miner calls sulphur rock, other people call it fool's gold, iron pyrites, and sometimes simply **[1407—1345]** sulphur. This is a familiar constituent of all coals; sometimes in small specks, so you cannot readily see them, but is always present to the extent of from 1 per cent up to 4 per cent of sulphur in this form. Oxygen of the air and the moisture combined bring about an oxidation of this material; it is rapid, if the coal warms up a little; it is a little slower if the coal does not heat. The oxidation of each per cent of sulphur represents an increase in weight of from 5 to 6 per cent in the coal. The oxidation of the carbonaceous matter is smaller in amount and probably only in a fraction of a per cent. But this process of oxidation begins most actively when the coal is put in storage.

(Testimony of S. W. Parr.)

Mr. ROCHE.—Q. You mean 5 to 6 per cent of the sulphur matter itself, and not of the coal?

Mr. SULLIVAN.—No. He says a combination of the oxygen with the sulphur will increase the weight 5 or 6 per cent.

Mr. OLNEY.—Just answer Mr. Roche's question.

A. If the sulphur which oxidizes is 1 per cent of the coal, the ultimate weight is 5 per cent of the coal.

Q. That is, by this process of oxidation, the sulphur increases in weight between 5 and 6 times?

A. Between 5 and 6 times; the exact figures are between 5 and 6 times the amount of sulphur involved, which would be between 5 and 6 per cent of the entire mass of the coal.

Q. When you speak of the sulphur involved, you mean the sulphur which is oxidized?

A. Yes, sir, which is oxidized. In using that illustration, I would not say that one per cent of sulphur does oxidize, I simply use it as an illustration.
[1408—1346]

Q. You are taking the case of a complete oxidation of one per cent of sulphur?

A. That is what I mean.

Q. Just explain a little more fully what the process of oxidation is, as you call it, just what goes on? Take the case of rusting, does it compare with that?

A. That will serve as an illustration. A mass of iron files which weigh to-day 10 pounds, at the end of the year may easily weigh 12 pounds, by reason

(Testimony of S. W. Parr.)

of the rust which has acumulated. The rust is a combination of the oxygen of the air with the iron. It increases in weight in proportion to the oxidation which has been going on. That is to say, there is no product of oxidation which is volatile and goes off in to the air. Now, precisely the same thing is true of the sulphur rock in the coal, or the sulphur crystals in the coal. The process of oxidation is simply the taking on of oxygen and making a new chemical compound, which weighs more than the original sulphur compound. It has an *altogether character* and a different weight.

Q. Are coals subject to spontaneous combustion?

A. They are, almost without exception.

Q. What relation does this matter of the oxidation of coal bear to this matter of spontaneous combustion?

A. Those coals which are most readily oxidized and are most susceptible to this process of oxidation, are the ones most likely to take fire spontaneously.

Q. Is the converse true, that coal which is subject or likely to spontaneous combustion, can you draw the inference from that fact that it is also peculiarly subject to oxidation? **[1409—1347]**

A. That is the fact.

Q. Do you *know the* reputation of Australian coals is in that respect, with regard to spontaneous combustion? A. I do.

Q. What is it?

A. It is among the worst cases of tendency to spontaneous combustion.

(Testimony of S. W. Parr.)

Q. And you would draw the inference from that with certainty that it is a coal that is particularly subject to oxidation? A. It is.

Q. To what extent would this factor of oxidation play a part in increasing the weight of coal during storage, say, for three or four months here in San Francisco?

A. It would play a larger part than under a short time storage. The conditions necessary for this oxidation are accessibility of oxygen and water; and the two things given a longer time will produce a greater oxidation.

Q. Let me put a specific case to you: Take the case of a vessel that was loaded with coal in, say, January, and the coal was kept under hatches for a year and a half, and during that time heated—at the end of that time it was found to have heated, and it was then discharged; within what limits would you expect a change in weight, an addition in weight to that coal, by reason of oxidation? This Australian coal, by the way, that you are dealing with?

A. I would say that the amount of oxidation would in all probability range somewhere between 2 and 4 or possibly 5 per cent. Those would be the extremes, I would say. **[1410—1348]**

(The witness was here asked to explain further the process of oxidation, elaborating upon the instance of iron filings.)

A. I think we are all of us familiar with the rusting of iron which is due to the addition of oxygen in that case and the result of the iron oxide is that

(Testimony of S. W. Parr.)

all of the iron is there and in addition there is oxygen which produces the rust; it is not a volatile constituent, and does not get away in the process of the chemical change, so that the actual weight of the iron rust is more than the iron with which we started it. This is true of all oxidation processes. The products of oxidation are heavier than the constituent which is being oxidized.

Q. For the process of the rusting of iron is it necessary that the iron come in contact with water or can it get this oxygen from the air?

A. It can get it from the air, but water facilitates the process. However, the water plays the part of a vehicle and is not necessarily involved in the ultimate result; it is simply a combination of iron and oxygen.

Q. Professor Parr, take the case of a pile of coal, say 8 or 10 feet high which begins to heat and into the interior of which pipes are driven and through those pipes water is poured into the interior of the pile from time to time to keep down the temperature; to what extent, if any, would changes of weight occur in connection with such a pile? **[1411—1349]**

A. The addition of water to coal that is heated increases the oxidation process, and a larger amount of oxygen is added than otherwise would be the case; it would not be at all out of the ordinary experience for such addition to amount to 4 or 6 or even 8 per cent; it would depend upon the time, and the length, —the amount of time involved in the heating process.

(Testimony of S. W. Parr.)

Q. Now, in connection with the changes of weight, what part, if any, is played by the relative humidity of the atmosphere?

A. On low moisture coals it cannot but add to the rate by absorption of moisture.

Q. Are there any changes in weight due to a change in moisture content caused otherwise than by the actual precipitation of rain on the coal?

A. There are.

Q. How would it take place—what is the cause?

A. The cause is due to the simple absorption of moisture in the air; a coal, especially if it is finely divided and dry has a very great capacity for absorbing moisture from the air.

(The witness Parr was here withdrawn for the moment by permission of the Court and consent of counsel for the prosecution in order that the defendants might put in some evidence as foundation for further questions to be asked of said witness.)

[Testimony of D. C. Norcross, for Defendants (Recalled)].

D. C. NORCROSS, recalled as a witness for the defendants, testified as follows: [1412—1350]

Examination by Mr. OLNEY.

I have prepared a statement of the coal stocks of the Western Fuel Company beginning with April 1, 1906, and ending with December 31, 1912, showing the amount of coal on hand at the beginning of that period and the amount of coal on hand at the end of each month thereafter. Said statement I now

(Testimony of D. C. Norcross.)

hold in my hand. In addition to the aforesaid information this statement gives the amount of coal received and sold, the balance and the inventory amount and some figures about overage.

(The statement was here offered in evidence, whereupon and before its receipt in evidence, Mr. Sullivan of counsel for the prosecution elicited the following testimony from the witness:) This statement was prepared about six months ago. Prior thereto I prepared another statement which showed a total overrun exceeding the total overrun shown in this statement. That statement did not show a total overrun of 10,000 tons in excess of the overrun shown on this paper. That statement showed, I think, 64,286 tons. It was a pencil statement. I may have the original. It was the first statement I ever prepared on this subject. The total overrun in this statement is 61,825 tons for the period from April 1, 1906, to December 31, 1912, and in the other and earlier statement 64,000 and odd tons. Mr. Tidwell and I agreed at the time when I prepared our first statement to check our figures. His statement was 50,000 tons and mine, as I have said, 64,000 tons. As we checked over together, we found differences on both sides. He brought his statement up to 62,000 and I brought my statement down to 61,000 odd. His statement and mine were approximately the same on the final check up.

The figure 64,000 tons was the one that I report to Mr. McNab before the indictments in this case were found. **[1413—1351]** The total overrun for the

(Testimony of D. C. Norcross.)

period indicated and set down in the statement I now hold, Defendants' Exhibit "HH," is 61,825 tons, that is, a percentage of 2.9 of the total amount of coal received and on hand. That includes all of the coal imported, both sold by barges or delivered through barges or through the yards to retailers. I am referring, of course, merely to imported coal. The percentage is on custom-house weights.

The average amount of coal on hand and on storage during this period on the first day of each month is 32,085 tons. The coal is stored partly in San Francisco and partly in Oakland. The Western Fuel Company has covered storage space affording protection from the rain for about 10,000 tons of coal, 6,000 in Oakland and 4,000 in the Folsom Street bunkers, but we only use about half the Oakland space for imported coal and, for the last two years half of the Folsom Street space has had the roof off. I should say that the total imports of the Western Fuel Company are made up approximately as to 70% of British Columbia coal, as to 25% of Australian coal, and as to 5% of Japanese coal.

Cross-examination by Mr. SULLIVAN.

The figures that I used in giving the aforesaid percentage of British Columbia coal were the out-turn weight, being 1,280,676 tons. The bill of lading weight would be 1,295,199 tons. The average amount of coal received per month would be very close to the sales, because there would only be a few thousand tons on hand at the end of the period, and it would average up about the same. The yard

(Testimony of D. C. Norcross.)

coal is apt to remain with us longer than the bunker coal. We always go to the bunkers [1414—1352] first and leave the yard pile untouched except when the bunkers are exhausted.

We do keep coal in the yard for say more than 30 or 60 days at a time. How long we keep it depends as a matter of fact upon the amount of coal in the bunkers. The coal is moving in and out a good deal from the Oakland bunkers as also from the San Francisco bunkers. The coal is also continually moving out of the yard in San Francisco, but it moves more frequently when there is no coal in the bunkers. Large quantities of coal are constantly coming in and going out and that is particularly true with reference to the bunkers which are merely temporary storehouses.

[Testimony of S. W. Parr, for Defendants (Recalled).]

S. W. PARR, a witness for the defendants, on the resumption of his direct examination, testified as follows:

Direct Examination by Mr. OLNEY.

I have made myself familiar with the records of the United States Meterological Station in San Francisco as to the falling of rain in this vicinity in the last 7 or 8 years, and I am also familiar with the records of said station as to the humidity of the atmosphere during the same period.

Q. Now, Professor Parr, take the case of a firm importing into this port and selling here during a

(Testimony of S. W. Parr.)

course of years, Australian, Japanese and British Columbia coals of the character of those you have examined, those low moisture coals, and assume, for the purposes of your answer, that the coals are discharged from the ships in which they are brought into bunkers, barges and yards, **[1415—1353]** and delivered from bunkers, barges and yards as sold by the importer, would such coals increase or diminish in weight between the time of their importation and the time of their sale, bearing in mind the climatic conditions existing here?

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The COURT.—I suppose that question contemplates either an increasing or a diminishing of weight under any and all circumstances.

* * * * * * * * *

A. It would increase in weight.

I am familiar with the bunkers, barges and yards of the Western Fuel Company and have been on the bunkers almost daily for the last month and have watched the coaling of ships. I have also been on the barges and on the ships and in the yards.

Q. Assuming that the Western Fuel Company was engaged in the business of importing and selling coal during the years 1906 to 1912, inclusive, and that during that period it imported or purchased after importation at the port of San Francisco, approximately 2,000,000 tons; that these coals were British Columbia, Japanese and Australian coals, in the proportion of about 70% from British Columbia, 5% from Japan, and 25% from Australia, and were of

(Testimony of S. W. Parr.)

the character of the coals you have examined here, and that in order to carry on its business, the Western Fuel Company kept in stock in its bunkers, including the Folsom Street bunkers and other bunkers, and in its barges similar to those now in use by it, and in its yards, an average of 32,000 tons of these coals during the period mentioned, and that of **[1416—1354]** these 32,000 tons not to exceed 7,000 tons were stored under cover, and that 25,000 tons were stored either in open bunkers or in barges or in yard piles, what changes, in your opinion, did those coals undergo while in storage?

A. The changes would be such as are due to changes in moisture and changes in oxidation.

Mr. OLNEY.—Would these changes affect the weight of the coal? A. They would.

Q. To what extent was the weight increased or decreased by these changes?

A. The weight would be increased.

Q. To what extent, in your opinion, was the weight so increased?

*   *   *   *   *   *   *   *   *

A. I should say that a very conservative estimate for the lower limit of increase, taking into account rainfall on the stock on hand at any period during the year, would be **[1417—1355]** 2 per cent. If I were obliged to figure on one quantity, I should make it more than that. I think that is a conservative figure. I think the upper limit, a conservative figure would be 4 per cent, or 4½ per cent. Within those limits somewhere, it seems to me, it is

(Testimony of S. W. Parr.)

easy to calculate the amount of weight taken on by rainfall.

Q. And oxidation? A. And oxidation.

I have visited the barges which were delivering the coal to Pacific Mail liners and have noted the character of the coal so delivered as to whether it was fine or coarse. I was present at the coaling of two separate ships.

Q. If such coal was so dry as to be dusty in the course of handling, what percentage in weight of water would be required to be added to it, in your opinion, in order to lay the dust?

A. I should say 2 per cent would hardly begin to lay the dust in a dry coal. I think that that could be made evident perhaps in this way: A ton of dry coal which would have two ordinary 3-gallon buckets of water turned upon it would not more than begin to moisten the coal; I doubt if it would lay the dust sufficiently to make it a working proposition. Two buckets of three gallons each would be a little over 2 per cent of water added to a ton of coal. I think to say that 2 per cent would be the minimum amount of water added to any amount of coal would be too small; but with somewhere between the range of 2 per cent and 4 per cent, the coal should begin to be dust free for working purposes.

Q. Would you say that 3 per cent would be a conservative **[1418—1856]** estimate?

A. I think it would be entirely conservative.

Q. Now, take the case of barges, and assume that in those cases where a barge was discharging coal

(Testimony of S. W. Parr.)

so dry as to be dusty and it was wetted down sufficiently to lay the dust, and assume that during one-half of the year, that is, during dry months, the coal discharged by the barges was of that character, and was wet down, and that the coal so delivered to the barges was approximately the same in amount, half year by half year, did such coal in your opinion increase in weight more or less than the general percentages to which you have already testified?

A. I think it would agree with the percentages I have given, entirely, with entire reason.

Q. That is, you think that the coal that was wetted down would increase 2 per cent—3 per cent?

A. At 3 per cent.

Q. The question which I asked you really was whether that would be in addition to the general percentage of from 2 to 4 per cent, or from 2 to 4½ per cent, to which you have previously testified.

A. I think it would, necessarily.

Q. To what extent would it increase those percentages, assuming that half the coal is wetted down in this fashion. I am referring now simply to barge coal; that is to say, we have from the rainfall, according to your statement, an increase in weight of from 2 to 4 or 4½ per cent; now, we have an artificial wetting going on during half the year on certain coals, the barge coal, and the barge deliveries being about the same, half year **[1419—1357]** in and half year out; now, I am asking you the per cent to which the barge coal would be increased in

(Testimony of S. W. Parr.)

weight, taking it all in all, by reason of the artificial wetting.

A. Well, you are asking me to distribute the percentage of the amount of barge coal to the total coal.

Q. No, I simply have reference to the barge coal itself.

A. It would increase it over the normal increase of atmospheric conditions to the extent of the 3 per cent distributed to the total percentage.

Q. And then if they wet but one-half of the barge coal, that would mean distributing the 3 per cent over the total of the barge coal, it would mean 1½ per cent? A. 1½ per cent.

Q. And that would make an overrun on the barge coals from moisture and oxidation and artificial wetting amounting to what?

A. If we figure the minimum of 2 per cent due to ordinary conditions, and 1½ per cent, that will make 3½ per cent for the minimum, and from 5½ to 6 per cent as the maximum, so far as the barges are concerned.

Q. Assuming that the barge deliveries during this period were approximately 700,000 tons, roughly speaking, can you figure out what the total increase in weight on all of the coal would be, that is, what the percentage would be on all of the coal due to this artificial wetting in the barges?

A. Yes, sir, that would be a simple matter.

Q. What is it?

A. 1½ per cent on 700,000 tons,—that is the num-

(Testimony of S. W. Parr.)

ber you mentioned, I believe. **[1420—1358]**

Q. Yes, and the total importation and handling is 2,000,000 tons.

A. It would be 0.52 per cent, a trifle over ½ of 1% on 2,000,000 tons.

Q. Considering the artificial wetting which goes on in the barges, the total per cent of gain which would come to all of the coal from moisture, from rainfall, humidity and oxidation, and from this artificial wetting, would be from 2½ to 4½ or 5 per cent, would it?

A. Yes, sir, within the range of from 2½ to 4½ or 5 per cent.

I visited Nanaimo in December last and made some experiments there in connection with the weighing of coal on scales. The coal there is weighed in trains of 20 cars.

In order to test the variation element of that scale, or any scale, it is necessary to see what a substance—what a mass—will weigh with the beam just free above, which would be called an upbeam, and just free below, which would be called the down beam, and the variation in weight of the mass would show the variation of the scale. That does not mean that it is a rising beam or a falling beam, but it means the beam which approaches the top and still does not touch, within the limits of being able to see daylight through the space; and the same thing with the down beam, which is not a falling beam, but which tests the delicacy of the scales with the beam near the low point. All of the cars, one at a time, were

(Testimony of S. W. Parr.)

passed over and weighed with this factor of the upbeam and the down beam, and the difference computed for the total mass of coal in the cars. **[1421—1359]**

Q. What did that difference amount to per car?

A. 117 pounds per car.

The approximate net weight per car would be 10,000 pounds. I made a comparison between the weights taken on an upbeam and the weights taken on a down beam and the weights taken on an exactly even poise. The difference between an upbeam and an exactly even beam amounted to 452 pounds; as between the even beam and the down beam it amounted to 1900 pounds; that is the percentage between the evn beam and the upbeam was .22; the percentage between the even beam and the down beam was .94; that is the difference on a whole train and not per car. The weight of the whole train on an even beam was 207,666 pounds.

I have made tests of the scales on the Folsom Street bunkers.

Q. What do you get there as the average difference between an upbeam and an even beam, an exactly even beam?

A. The difference in percentage is 0.20 of 1 per cent, weighed as I have described, not as a rising beam but as the upper limit of accuracy with the beam not touching above.

Q. What would be the difference between an even beam and a down beam, for instance, on those scales?

A. 0.18 of 1 per cent.

Q. Suppose the weighing were with what may be

(Testimony of S. W. Parr.)

called a rising beam, that is to say, the beam starts to rise from the bottom, and is caught, and comes up at such a rate that the weigher thinks it will come to a rest somewhere between there and the top, and he clamps the scale and catches the weight quickly, would **[1422—1360]** that process increase or diminish the difference between the weights which you got with an upbeam, as you call it, and an even beam?

A. It would very materially increase the difference.

Q. Did you notice any other factors at the bunkers or any other thing at the bunkers which would increase the difficulty of weighing or increase the factor of error due to weighing with a rising beam?

A. Probably the vibration to which the mounting of the scale is subjected would be the most serious source of error.

Q. What were those vibrations due to, or, rather, when did they take place, when did you observe them?

A. The most evident vibration is due to the passing of the train under the weighing-house; wind currents are serious sources of error, also. I think those two are the most noticeable sources of error in the mounting of the scales on the bunkers on Folsom Street.

A bucket loaded full and containing lump and fines mixed as they come will weigh more than a bucket containing simply fine coal. I made tests here to ascertain whether that is the fact. First I filled a

(Testimony of S. W. Parr.)

tub with screenings or fine coal only, as it came from the belt on the bunkers. This bucket weighed 921 pounds. The same bucket filled to the same height with lump and fine coal mixed weighed 1098 pounds, being a difference of 177 pounds. Those were weight of the coal only. The percentage of difference is 19.16. I also weighed a bucket made up wholly of lump coal. That bucket was heaped somewhat higher—its weight was 910 pounds net, being less, therefore, than either of the other buckets. **[1423—1861]**

BE IT REMEMBERED that thereupon the following testimony was given and that the following proceedings occurred:

Q. In addition to the weighing of the tubs down there at the barges, did you make any experiments or tests where you could get at the matter more accurately, that is, estimate the quantity of the coal, the cubic contents of coal, more accurately, and then weigh them?

A. We have a number of tests in which we use a straight edge to bring the tubs to a level, which would not depend simply upon the eye, to gauge the variation in weights of tubs under more exacting conditions of filling.

Q. Let me ask you this question first: As a matter of fact, you took a laundry tub and a can, did you not, and made the same experiments?

A. Yes, sir, so that the measure could be a struck measure; a tub, for example, of that sort, which would weigh 91 pounds of coal, fine coal struck level,

(Testimony of S. W. Parr.)

with lumps and fine, would weigh 113 pounds, being an increase of 22 pounds, or a variation of 24½ per cent as between the fines, only, and the lump and the fines. That experiment was repeated sufficiently to make it evident that the range was somewhere easily between 17 per cent and 24 per cent, depending on the amount of lump and the amount of fines. The higher percentage, that is, 24 per cent, was in a case where the fines amounted to about 50 per cent, half and half, half lump and half fines. The garbage can repeated those experiments, because the struck measure could be a little more easily secured, and the variation in percentages between fines and lump ranged between 15 per cent and 19 per cent; the mixture of lump and fine being 15 per cent heavier than the fines alone, or 19 per cent **[1424—1362]** heavier than the fines alone.

Q. Did you make any experiments or tests with the buckets on the barges, to ascertain how full or what weight of coal they had to contain in order to tip? A. I did.

Q. Will you state what you did, Professor Parr, and also state what you found?

Mr. ROCHE.—Do I understand you to claim that the witness personally made the experiment?

Mr. OLNEY.—Yes, he was there.

Mr. ROCHE.—Or merely watched them?

Mr. McCUTCHEN.—He directed them.

Mr. SULLIVAN.—Did he attend the engine?

Mr. OLNEY.—He supervised it.

Mr. SULLIVAN.—We object to that on the same

(Testimony of S. W. Parr.)

ground that we objected a little while ago, it amounts to self-serving conduct on the part of the defendants. We don't know how the engine was handled by the man in charge, or how the hatch-tender performed his duty. All this witness saw was what we ourselves saw there the other day. He saw the tub containing a certain quantity of coal, and he saw them tip the tub in the bunker; that is all he can testify to.

Mr. OLNEY.—He saw more than that, Mr. Sullivan.

Mr. SULLIVAN.—We submit that he should not be allowed to testify to that matter unless we know just how the engine was manipulated.

Mr. McCUTCHEN.—One of these witnesses testified that he was on the bridge of the "Korea" on the night of the 18th of December, and that he saw tubs which were not weighed, which were only half filled, discharged into the chute; and he said in another case he could see down at least one foot [1425—1363] into the interior of the tub, and that such tubs were discharged into the chute—

The COURT.—Didn't we see these experiments ourselves the other day? What was the use of going down there and watching those things, if we must now have a witness come in to testify to them, and in addition to that, under circumstances we are not acquainted with?

Mr. McCUTCHEN.—The only point is, if your Honor please, that he made the measurements.

Mr. OLNEY.—He made the measurements, and he got the weights and the portions.

(Testimony of S. W. Parr.)

Mr. McCUTCHEN.—Your Honor and the jury didn't make the measurements the other day. We want to show by the witness that a tub that is so empty that a witness could see into it one foot could not actually be dumped.

The COURT.—Didn't we see that ourselves and find out we had to smash things to do it?

Mr. McCUTCHEN.—I don't know to what height the tub was filled on those occasions.

Mr. STANLEY MOORE.—And in regard to the running of the engine, your Honor, I am informed that they banked away there until they knocked the whole rigging down, so it would not make much difference whether it was Eddie Powers running the engine, or somebody else; on the occasion of these experiments, he even outdid Eddie Powers.

Mr. SULLIVAN.—We object to any experiments made for the benefit of this witness' observation. We have all seen just exactly how the engines are operated and how the tubs are operated.

The COURT.—The objection is sustained. **[1426—1364]**

Mr. OLNEY.—We note an exception.

On more than one occasion I went down to the barges to make tests for the purpose of ascertaining whether or not these tubs would tip. In the course of making those tests I endeavored to fill the same bucket at different times to exactly the same height. I was particularly careful in that regard.

Q. What differences in weight did you find in

(Testimony of S. W. Parr.)

those buckets, or between the same bucket weighed at different times, and loaded each time, as you say, to the same height, as nearly as you could get it?

Mr. SULLIVAN.—With the same kind of coal, or different kind of coal at different times?

Mr. OLNEY.—Q. Was it the same kind of coal?

A. It was. On the "Theobold," tub No. 2 was taken and filled, seven tests being made, the filling being as nearly exactly to the same piont each time, with weights as follows: net, 921, 1098, 1034, 1041, 1084, 1053; I think I should omit the seventh one; I said there were seven, but the seventh was lump, only, and it weighed 910, but the filling was higher, because of the difficulty of getting lump, coarse coal, or to see whether we could put on enough to make it weigh as much as the fine coal.

Mr. SULLIVAN.—Q. How much did that weigh?

A. 910 pounds. Then on the "Comanche" one weight was 1390 net, another was 1340. Tub No. 2 on the same barge—

Mr. SULLIVAN.—Q. That was No. 1?

A. Yes, No. 1; the first "Comanche" experiment with tub No. 1 gave weights 1390 and 1340. Tub No. 2 gave weights of 1400, 1360, 1370, 1400. Tub No. 4 on the barge "Nanaimo" gave 966, 986, 930. Those are all the weights that I **[1427—1365]** have. I will say that these tubs on the different barges are not comparable, the tub on one barge is not comparable with the tubs on another barge, necessarily.

Mr. OLNEY.—Q. Did you observe the amount of coal which could be added to or taken from a tub

(Testimony of S. W. Parr.)

apparently full without changing the practical appearance of the tub? A. I did.

Q. What would it amount to?

A. It would seem evidence that two scoopfuls, for example, of coal, spread over the area of the top of a tubful would hardly be noticeable by the eye. Two scoops would amount to about 40 pounds easily on the average; so that I do not believe the eye could detect a variation of that amount. I think three or four scoops would make enough of a difference in the surface level of a tub to make it evident to the eye.

Cross-examination by Mr. SULLIVAN.

I am now, and have been for a number of years last past, on the staff of professors at the University of Illinois. In a certain sense I am also at the present time employed by the Western Fuel Company as an expert. The first work that I did for this company was last August. The individual who saw me at that time on behalf of that company was Mr. Mannon, an associate of Mr. Olney. My compensation was to be a per diem. The gross amount of my retainer fee was not determine. The most important consideration to me was the stipulation that I should find the facts in the case without regard to whom they would favor. My per diem was $25. I was to be paid in **[1428—1366]** addition for such analytical work as I should do, but I have not yet made up my bill for that work. The per diem has not been running continuously since last August. There has been a gap of about one hundred days. I

(Testimony of S. W. Parr.)

usually charge $10 per sample for analyses and I have worked up about 10 samples for the Western Fuel Company.

The first work I did for the Western Fuel Company was to go to the mine at Nanaimo and take samples of the coal. I was there about three or four days. I went into shaft No. 1, a distance of approximately a mile. That was in the North Workings. That section of the mine was at that time in operation, but no coal was taken out on the days that I was there. Professor Sommermeier of the State University of Ohio was with me.

We took out 70 or 100 pounds of coal from the face of the vein, broke that down, sampled it and took away with us about a 5 pound sample, being the amount prescribed in such cases by the bulletins of the United States Government. I did not pick out that 5 pounds for a sample. I took it out according to the prescribed methods.

Q. What do you call the prescribed methods?

A. It is quite an item of detail.

Q. In general terms can you tell us?

A. In the first place, it is to break the lumps to a uniform size, heap them in and cone and quarter them, so that you will take alternate quarters or opposite quarters; in this case you would then have a 40 lb. sample, we will say. It is further broken down, so that now the pieces are perhaps the size of a walnut, and they are mixed and coned and quartered and the opposite quarters rejected. These quarters remaining are further [1429—1367]

(Testimony of S. W. Parr.)

crushed. This is all done on a canvas or oil-cloth surface, so that absolutely no coal will get out of place or be lost. It is again mixed and coned and quartered until the sample obtained is a fair accurate representative of that mass to begin on, amounting to about 5 lbs. This is taken to the laboratory; it is first sealed in a screw top can, wrapper thoroughly with electric tape, so that there can be no changes in moisture content. That is in brief the method as outlined in the bulletin.

The coal was crushed in the mine itself at the face of the vein and was there segregated into the 5 pound sample. I took said sample in the can with me east and there analyzed it. I took no samples from the bunkers at Nanaimo, but I did take some from the cars on the track. There were, I think, 14 cars on the track at the time. I took samples from at least two and I had three samples in all. No one of those samples was taken from more than one car. The method of sampling was exactly a duplication of the method pursued at the face of the vein and which I have heretofore described. These car samples were also put in air-tight cans and taken east by me to my laboratory. I have no personal knowledge as to the duration of time during which the coal in the cars had been exposed to the weather before I took the samples. There was no rain while I was in Nanaimo. The sun was shining most of the time. There was evidence of rain, however, on the coal that I worked with. I don't know whether they played the hose on the coal or whether the water came from

(Testimony of S. W. Parr.)

the heavens above. I did not see any hose in proximity to the cars at any time. I did not notice any rain around on the streets or houses at Nanaimo. I am certain, however, that I found water in this [1430—1368] coal on the cars. I have a report of the examination that I made on the samples that I took east. One of the sheets is already turned in as evidence. It covered both the coal taken from the cars and from the mine. It recorded an experiment to show the water capacity of the coal at different seasons. The Nanaimo coal is a low moisture coal, by which I mean that it naturally has in itself a comparatively slight amount of moisture. All of the coal which I got at Nanaimo was of this type.

Q. How was that moisture in the coal, is it in chemical composition or is it simply there in its natural state—is it there in the separate gases or in the natural state of water?

A. The story as to how water is in coal is rather a long and complicated one.

*   *   *   *   *   *   *   *   *

It may be in the form of excess water which you can see, and the coal looks wet; it may be in the spaces of the coal, the texture of the coal, so that the coal will look dry and be dusty; but it may be as high as 14% of water under different conditions; it may be a chemical constituent of the coal which does not appear under any of those heads.

Mr. SULLIVAN.—Q. Well, isn't it generally found by the chemist to be in the form of gases, which is hydrogen and oxygen?

(Testimony of S. W. Parr.)

A. That depends on what he is working for; if he is working for the chemical compounds, he would get it in a very different way.

Q. Don't you always make an analysis of coal to **[1431—1369]** ascertain the amount of hydrogen and the amount of oxygen, the amount of carbon in the coal, and sulphur, and the other constituents of the coal?

A. That is true in the case of certain work where the engineer wishes those constituents. It is not very frequently done.

* * * * * * * * *

Q. Now, when you took this coal with you to the laboratory, did you dry it at all?

A. When it was unsealed from the can it was weighed, immediately, before there could be any change in moisture content; then it was exposed to the air until it had dried out to a point where it would not lose any more water.

Q. Was it exposed to a warm air or air of ordinary temperature in the room where it was, that would be in the ordinary room of the laboratory?

A. That would be a matter of indifference, either way; a slightly elevated temperature, with a current of air moving over it would be one method, or it could stand out in the air and allow the water to evaporate by itself; the only point being to weigh the coal after it had been air-dried to see what was the amount of water which would go off readily in the process of drying in the air.

Q. Now, was this coal put in a dry room for the

(Testimony of S. W. Parr.)

purpose of allowing the moisture to evaporate from it?

A. Well, I hardly know what you mean by a dry room.

Q. You say it was air-dried, was it not?

A. It was air-dried.

Q. What was the amount of humidity in the air there as compared with the humidity in another room of **[1432—1370]** the laboratory?

A. The humidity may have been lower; it probably was.

Q. Isn't it purposely made lower for the purpose of drying the coal out?

A. So that the time could be cut down, yes.

Q. How long had it been exposed to this air?

A. 24 hours.

Then we weighed the coal. These coals drop in the process of air-drying on an average of ½%. We had the coal in the can probably 2 weeks. It was exposed to a temperature slightly higher than the normal temperature at that time for 24 hours to dry it out, and, after that exposure, I noticed a depreciation in weight of the coal equivalent approximately to ½ of 1%, by reason of the loss in moisture during that period of time which represents the loss of moisture in the coal from the time the can was opened until the air-drying process was complete.

Mr. SULLIVAN.—Q. Suppose that coal was left in an open can instead of being put in a sealed can, air-tight can, would it weigh more or less at the time

(Testimony of S. W. Parr.)

you exposed it to this extraordinary temperature, or at the time it was weighed at the laboratory?

Mr. McCUTCHEN.—The witness hasn't said that it was exposed to any extraordinary temperature.

Mr. SULLIVAN.—He said it was not the ordinary, and if it was not the ordinary temperature, it must have been extraordinary temperature.

A. Precisely that same method of getting at the air-dry loss is obtained by leaving it perhaps two days **[1433—1371]** in the atmosphere of the laboratory, but that amount of moisture is immaterial to the chemist. He only wishes to bring the coal to a point where, when he works with it it will not take on more water, as he weighs it, because it is extremely sensitive to moisture changes, or it will not lose moisture; he only wishes to bring it to a point which is the nearest equilibrium to the moisture in the atmosphere, so that when he works upon it from day to day it will not vary in his hand. Now, that is an indifferent point. There are still 2 or 2½ per cent of moisture in the coal which he goes after later. This point that you are striving at now has no relation to the coal, and it is an indifferent point to the chemist; it is a point as near the equilibrium as he can get the coal for careful work. If a can were left open and transported from Nanaimo to the laboratory, you would be obliged to have an accurate weight at the mine upon scales which would weigh to one part in 10,000; that would be impractical; the nearest we can do is to seal it up and prevent changes of moisture until it gets to the laboratory. There

(Testimony of S. W. Parr.)

we can reach these totals with care.

Q. Suppose that can were opened and the coal were allowed to remain in the can for 24 hours, say in an atmosphere like the atmosphere in this room here now, how much moisture would there be lost, do you suppose?

A. It might amount to one-tenth of one per cent; I can only guess at it.

Q. It might amount to one-tenth of one per cent. Suppose it was out in the air?

A. It would amount to more.

Q. It would amount to more on account of the wind and a greater amount of evaporation?

A. Certainly it would.

Q. So, then, the coal would become lighter by reason **[1434—1372]** of that exposure to the air either in the room or on the outside?

A. It would.

Q. By reason of a certain quantity of moisture?

A. Yes.

Q. Is this moisture visible to the eye that you can notice in the coal after subjecting it to the temperature for 24 hours? A. It is not.

Q. It is apparently, if it is in the coal at all, it is in a chemical combination, is it not? A. It is not.

Q. How is it, then, you cannot see it; can you squeeze it out of the coal or crush it out of the coal?

A. You can't.

Q. If you can't crush it out of the coal, how do you know the water is there?

A. By weighing it accurately before putting it into

(Testimony of S. W. Parr.)

a drying-oven, and weighing it after it comes out.

Q. Let me understand you: you get this coal, weigh it, as soon as you take it out of the can, subject it to more than ordinary temperature for 24 hours, then weigh it again, and it weighs less, one-half of one per cent less, you come to the conclusion that the loss is loss of moisture, don't you? A. Yes, certainly.

Q. Suppose you had taken that coal after 24 hours' exposure to this atmosphere and crushed it and pressed it in any conceivable form, could you squeeze any water out of it? A. You could not.

Q. Isn't it a mere guess on your part, that **[1435—1373]** this loss of substance is a loss of water? A. It is not a guess.

Q. It is not a guess? A. No.

Q. Well, is there any mechanical process known to the chemist or anyone else or to any mechanic, by which you can take 5 lbs. of coal exposed for 24 hours and get the water in its normal condition out of that coal?

A. That is one of the every day occurrences in the laboratory; it is one of the easy things to do.

Q. How do you do it?

A. By putting the coal in a closed vessel and conducting the gas as it comes off through a condensing apparatus so that the water will condense in the form of a drop of moisture or in a substance where the water will be taken up and weighing the actual water that comes off from the coal.

Q. Does not that prove that the water is due simply to the presence of oxygen and hydrogen in the gas

(Testimony of S. W. Parr.)

that may not be united in the coal?

A. I cannot consent that oxygen and hydrogen are water.

Q. What makes water? What are the constituent elements of water?

A. Hydrogen and oxygen in combination.

Q. That is what I said, isn't it?

A. Absolutely not.

Q. Oxygen and hydrogen in combination make water, don't they?

A. If they are in chemical combination.

Q. In chemical combination A. Yes.

Q. Now, will you explain to the jury how **[1436—1374]** it is you cannot see the water after this exposure, and you do see water after you subject it to a certain amount of heat or a certain process?

A. You cannot see the moisture in this air, can you? But if it gets enough to form raindrops you can see it. That is a simple illustration.

I have never before testified as a witness on behalf of the defense in any case where the Government was prosecuting parties for defrauding it out of duties.

I have several times analyzed coal for the purpose of determining the amount of oxygen and hydrogen in it. Both oxygen and hydrogen in coal weigh something.

Q. They are both gases?

A. Not in coal; they are not gases in the coal.

Q. You say they are not gases in the coal?

A. They are not.

(Testimony of S. W. Parr.)

Q. If, by any mechanical process you can discover water in the coal, how is it you are able to say there are not gases formed in the coal?

A. Hydrogen and oxygen constitute sugar. Sugar is not a gas.

Q. I am talking about the separate gases.

A. But they are not separate in coal.

Q. Are you sure of that?

A. Why certainly I am.

Q. Why do they conclude that they escape in the form of water by reason of the drying process before you weigh the coal a second time, after having weighed the coal the first time?

A. May I ask if you mean why the hydrogen and [1437—1375] oxygen escape?

Q. No; why do you conclude that the hydrogen and oxygen escape in the form of water before you weigh the sample the second time?

A. Because if I have any doubt about its being water I could collect it in such a form as to test it and whether it was water or oil or any other substance.

Q. Did you make those tests in this particular case?

A. I did not.

Q. You did not make that particular test in the case of any one of these five samples, or four samples rather? A. As to whether it was oil or water?

Mr. McCUTCHEN.—The witness asks you a question, Mr. Sullivan?

Mr. SULLIVAN.—Q. I say did you make this particular test by which you could determine abso-

(Testimony of S. W. Parr.)

lutely that it was water in the case of any one of these four samples you analyzed?

A. The method of analysis was such that it could not have been anything but water.

Q. That is not answer to the question, Professor.

A. Well, indirectly I did make that test; a chemist would say,—

Q. Indirectly you did make the test. What was the test you made?

Mr. OLNEY.—What were you going to answer. A chemist would say what?

A. A chemist would say that the method he employed for determining those constituents [1438—1376] would show that it couldn't have been anything but water, hence that would prove that it was water.

Mr. SULLIVAN.—Q. You say indirectly you made a test. Explain the manner in which you made this indirect test.

A. The coal was dried at a temperature which would liberate the water, and a temperature which is especially adapted for the liberation of water—

Q. What was that temperature?

A. It was 5 degrees above the boiling point of water at the altitude where the work was done.

Q. Five degrees above the boiling point?

A. Two hundred and seventeen degrees Fahrenheit.

Q. You have analyzed quite large amounts of bituminous coal of the eastern estates, have you not?

A. I have.

(Testimony of S. W. Parr.)

Q. How does the moisture content of bituminous coal in the east compare with the moisture content of coal from Nanaimo?

A. We call the east, east of the Alleghany Mountains; Pennsylvania and West Virginia and Maryland though are almost identical in water content with the coals of Nanaimo.

Q. How are they in respect to other constituents?

A. Some of those same coals are very similar in the amount of gaseous or volatile matter which those coals may be made to give off; others are lower in that constituent.

Q. How does the amount of sulphur in the Nanaimo coal compare with the sulphur in the eastern coals?

A. It **[1439—1377]** is within the range of variation of sulphur in the eastern coals.

Q. So that the eastern bituminous coal is practically the same as the Nanaimo coal; is that so?

A. Very many of them are; not all of them by any means.

Q. Well, the average eastern bituminous coal.

A. No, not the average; coal from specific districts are very similar to Nanaimo coal.

Q. Is it not the invariable rule that coal exposed to the atmosphere for any length of time will lose in weight? A. It is not.

Q. Is it the invariable rule that coal exposed to the atmosphere will gain weight? A. It is not.

Q. What is the rule as to gain or loss of weight by exposure to the air?

(Testimony of S. W. Parr.)

A. There is no rule, it depends upon the quality or type of coal and the atmospheric conditions.

Q. Is not the loss of coal generally complained of by the railroads in the east due in a great measure to evaporation of moisture in the coal when stored?

A. Those coals that I have described do not lose *the* evaporation to any very great extent. If there is a loss on high moisture coals it is ascribed to evaporation. Many of those coals are given a tolerance on account of moisture, ranging from 1 to 15 per cent.

Q. How does nut coal compare with the bituminous coal? Is nut coal found in different kinds, bituminous and anthracite, or is nut coal a special quality of coal?

A. It is not a special type of coal; it is a kind of any type. **[1440—1378]**

Q. It may be a kind of bituminous coal or a kind of anthracite coal. A. It may.

Q. It is termed nut, on account of its shape, I suppose, the shape of the particules of coal; is that it? Why is it called nut coal?

A. It is not a definite term, I would hardly know how to define it; approximately walnut size perhaps would be the commonest interpretation of the term.

Q. What is the kind of coal that is found in Vermillion County, Illinois, bituminous coal or anthracite? A. Bituminous.

Q. And how does that bituminous coal compare with the kind of coal that is found in Nanaimo?

A. There is no comparison, they are not the same type of coal at all.

(Testimony of S. W. Parr.)

Q. What is the moisture content of that coal as compared with the moisture content of Nanaimo coal?

A. It will vary from 13½ to 14½ per cent as it comes from the mine.

Q. That is, that coal is a high moisture coal?

A. It is a high moisture coal.

Q. And what kind of coal is that found in Williamson County, Illinois? A. It is a bituminous coal.

Q. It is a high moisture coal or a low moisture coal?

A. It is a high moisture coal but not so high as the Vermillion County coal. It will average from 10 to 12 per cent in moisture.

Q. Does that coal increase in weight or decrease in weight by exposure to the atmosphere for any length of time under ordinary conditions?

A. If it is shipped in open cars from Vermillion County, a distance of 35 miles, coal **[1441—1379]** which has been under my inspection for 50 months, for all that time the variation in moisture loss or gain has been less than a half per cent. The same coals however, shipped from Williamson County, a disance of 175 miles, without rain upon them, and only exposed to dry weather conditions will lose moisture sometimes to as much as 2 per cent.

Q. Assume that the coal is left in an exposed bin for a whole year, will it increase or decrease in weight?

A. I have just completed experiments or six carloads of coal stored in bins such as you describe; those

(Testimony of S. W. Parr.)

coals from Williamson County went in with a moisture content of 10 per cent. They now have a moisture content of nearly 20 per cent—19 and a fraction.

Q. How long were they exposed?

A. They have been exposed six years to rain and weather, drying out and wetting up.

Q. What was the total increase in weight?

A. The total increase in weight was approximately 9 per cent.

Q. The coal from Vermillion County also increases in weight, does it, when exposed in an uncovered bin for a year or more?

A. It would be subjected to the same law as governed in the case of the Williamson County coal; in fact there is a bin of the same size side by side with this other experiment.

Q. There would be a like increase?

A. There would be.

Q. And take the coal from Sangamon County, Illinois; is that a high moisture coal or a low moisture coal? [1442—1380]

A. It is a high moisture coal.

Q. And will that increase in weight in an uncovered bin, in an exposed bin, for a year?

A. It depends entirely on the weather conditions.

Q. Well, then, take the ordinary normal weather conditions. A. If it is a fine coal—

Q. Well, say, take nut coal. I am referring to nut coal now in these different case.

A. It would increase in weight.

Q. It would increase in weight, to what extent?

(Testimony of S. W. Parr.)

A. Over a period of one year it would be difficult to say, possibly 3 or 4 per cent.

Q. Increase? A. Increase.

Q. Would screenings of the Vermillion County coal increase in weight or decrease in weight if exposed for one year in an open bin?

A. They would increase.

Q. To what extent?

A. If you mean by screenings a finer coal so far as divisions are concerned than the nut coal they would increase more than nut coal, possibly 5 or 6 per cent.

Q. To what extent will they increase?

A. Perhaps 5 or 6 per cent.

Q. And if those coals in these different counties were stored in Government bins would they increase or decrease in weight?

A. If they were stored in bins in which there was no access of water, the chances are they would lose in weight.

Q. Now, take the screenings from Williamson County, **[1443—1381]** if those were exposed for a year in an uncovered bin, would they increase in weight or decrease in weight?

A. The ultimate result would be a decrease owing to the fact that there is more moisture to be lost than oxygen and other conditions to be gained.

Q. Where the bin was exposed?

* * * * * * * *

A. Understand the question to be as to a covered bin.

(Testimony of S. W. Parr.)

Mr. SULLIVAN.—Q. Assuming the screenings from Williamson County were in an exposed bin, would they increase or decrease in weight?

A. They would increase in weight.

Q. To what extent? A. Possibly 5 or 6 per cent.

Q. Would you say that also about screenings from Sangamon County, stored in an exposed bin?

A. They would increase in weight.

Q. Does coal increase or decrease in weight by reason of oxidation?

A. The result of oxidation is always an increase.

Q. Is this a correct statement of the fact, Professor:

"The storage of coal has been thought by some investigators to result in a considerable loss in weight. If the coal heats considerably and fires occur in the storage piles, there is undoubtedly a great loss in weight, but it is still an open question as to how great this change is in storage under fair average conditions. The change in the weight of air-dry or wet coal cannot be considered in this connection. The exact weight of dry coal must be ascertained by some means. The coal must be weighed and then dried in an oven at some certain temperature **[1444—1382]** for an exact determination of the amount of moisture in it at the time, as it is not possible to get the coal into such an air-dry condition before and after storage that it will contain exactly the same percentage of moisture each time."

This refers to certain experiments made:

"The coal in these experiments was weighed, sam-

(Testimony of S. W. Parr.)

pled carefully, and the moisture determined at 105° C. The weight of the moisture was then deducted from the weight of the air-dry coal, and the resulting weight of dry coal was used in making all comparisons. The results are given in Tables 3, 4 and 5. The larger samples (Table 3) show a loss of about one and one-half per cent in weight in two cases out of six, the other four show no change within the limits of experimental error, and it is doubtful whether even these two are not chargeable to possible discrepancies in moisture determinations rather than to any actual change in weight. The results of these experiments show only that any change in weight that does occur is very small in amount and is not to be considered unless the coal has heated considerably."

Is that a correct statement of the proposition?

A. It is, and is entirely consistent with what I have been saying. You are talking about coals which heat in storage and put where there has been active combustion.

Q. Now, I show you some tables here of analyses of coal that was analyzed, coal taken from Vermillion County, nut coal, Williamson County and Sangamon County, and screenings coal.

Mr. OLNEY.—What is that you are reading from, Mr. Sullivan? **[1445—1383]**

Mr. SULLIVAN.—This is Professor Parr's work on "The Weathering of Coal."

Q. I show you table 6, showing the analysis of coal made.

(Testimony of S. W. Parr.)

A. What is the particular point you wish me to look at?

Q. Does not that table show that nut coal of Vermillion County exposed in a bin, an open bin for a year decreases in weight a certain percentage?

A. You mean by this column here (pointing)?

Q. Yes.

A. That column is a measure of the heat in the coal and it is headed at the top, a decrease of these various percentages, 1.06, 1.06, 2.13. That is a decrease in the heat units of the coal.

Q. And not in the weight of the coal?

A. There could be no decrease in the heat unless there was an increase in some nonheat producing substance which in this case is either oxygen or water; so that that column verifies the statement I have already made that there is an increase in weight of some noncombustible which decreases the heat capacity by those figures.

Q. Did you make a memorandum at that time showing the increase or decrease in the coal?

A. I would not have to; it is on the face of it.

Q. There is nothing on the face of that showing the increase or decrease of the coal, is there?

A. There is to a chemist, certainly.

Q. Does this *decree* at the head of the column refer to decrease in the heat power of the coal only?

A. It does, to the heating power of the coal only; **[1446—1384]** to the number of heat units in the coal. It is a heat quantity decrease and not a weight decrease; the two things are opposite.

(Testimony of S. W. Parr.)

Q. Has it not been your experience, Professor, that coal stored in large quantities will evaporate to such an extent that there is considerable loss of weight in the coal?

A. I have had no such experience but the contrary is true.

Q. Does it make any difference whether the exposure is in dry weather or wet weather?

A. That depends, of course, on the size of the coal stored.

Q. If coal is stored in dry weather for six months, will the storage result in loss of weight by reason of evaporation or will the storage result in increase of weight by reason of oxidation?

A. What kind of storage do you have in mind?

Q. Say yard storage or open bunker storage?

A. And you are asking as to what will be the effect of oxidation?

Q. No, I am asking you whether coal exposed in dry weather for the period of six months will increase in weight or decrease in weight?

A. There are two processes going on,—the oxidation process may be overmatched by the volatilization process; it would be impossible to say.

Q. Cannot you give me an answer? Say, take the ordinary bituminous coal containing the elements the Nanaimo coal contains, if that is exposed to dry weather for a period of six months can you answer me this question, will it increase in weight or decrease in weight by reason of the storage? **[1447—1385]**

(Testimony of S. W. Parr.)

A. If it is a type of coal like the Nanaimo coal there will almost necessarily be an increase in weight by the storage, basing my answer on the theory that it is coal of low moisture content.

Q. So that if this coal is contained for six months in an inclosed bin, during ordinary dry weather, would that coal increase in weight or diminish in weight?

A. It would be impossible to say without knowing the size of the coal, and the humidity—

Q. (Intg.) In the hypothetical questions which you answered this morning, the size of the coal, if I remember rightly, was not given.

A. I beg your pardon.

Q. I did not understand that the size of the coal this morning in the hypothetical questions propounded to you was given.

Mr. McCUTCHEN.—The hypothetical question asked the witness to deal with the coal such as he had seen in the Western Fuel Company bunkers.

Mr. SULLIVAN.—Q. All right. Take the coal that you have seen, the Nanaimo coal, and assume that that is stored in a closed bin in quantities—

Mr. OLNEY.—Here in San Francisco?

Mr. SULLIVAN. — Q. (Continuing.) — In San Francisco, yes, for a period of time, in dry weather, would that coal increase in weight or decrease in weight? A. It would increase in weight.

Q. Assume that the coal was concealed, or suppose it was placed in the hold of a vessel, and the hatches were closed, for a period of six months, would it in-

(Testimony of S. W. Parr.)

crease or decrease in weight.

A. It would increase in weight. [1448—1386]

Q. Suppose it were inclosed in the hold of a vessel instead of six months, for six days, would it increase in weight or decrease in weight?

A. Theoretically it would increase; it would be a difficult matter to measure the quantity of increase in six days.

Q. Would that increase in weight be perceptible for the period of six days?

A. I am unable to say.

Q. Well, what is your best opinion as an expert?

A. You are measuring quantities of such small amount that it is difficult to say. If it would increase in proportion to the time I should say the increase would be evident, there would be an increase, whether it would be evident or not, would be another question.

Q. Now, take 3,000 tons of coal and put it in the hold of a vessel four days, and close the hatches; that coal you say would increase in weight during those four days, would it? A. It would.

Q. And that increase in weight would be a proportionate increase, would it not; that is, in proportion to the weight if the coal had been kept six months instead of four days?

A. It might, or it might not.

Q. In your opinion it might; is not that so?

A. It is possible that it would be a proportionate increase.

Q. Is it not probable that it would?

(Testimony of S. W. Parr.)

A. I cannot say. There would be other conditions that would enter into that that I would have to know before I would commit myself. [1449—1387]

Q. You answered the questions this morning involving all of the coal handled by the Western Fuel Company since 1906 very positively, did you not?

A. I think I did.

Q. And you cannot give a positive answer to the last question which I propounded?

A. You have not placed the conditions sufficiently accurate to make it possible to answer it positively.

Q. In answering the questions this morning you didn't know how much coal had been kept in the yard without being moved from any particular place, did you?

A. I did, in that I knew the gross amount for a month, month by month.

Q. That is, you know the amount that had been moved month by month?

A. I did, the average amount month by month.

Q. Did you understand that the coal was kept in motion all the time by reason of sales and shipments?

A. It didn't make any difference whether it was kept in motion, or not, so far as my opinion was concerned.

Q. So that coal, whether kept in motion or not, or whether stationary or not, according to your theory, will always increase in weight from the time it is commenced to be handled up to the time it is consumed; is that so?

(Testimony of S. W. Parr.)

A. As a man stands on top of the bunkers on Folsom Street and watches the coal in motion during a rainfall, he has no question about the fact of increase in weight while the coal is in motion. The same is true if the coal is standing still, he has no question about the increase in weight.

Q. Supppose a man stands on the bunkers and goes down there and stands on the bunkers every day for six months, during which six months there is not a particle of rain, and **[1450—1388]** he sees the coal come and go, do you say that during that period of six months that of necessity there would be an increase in the weight of the coal?

A. There would be, small in amount.

Q. By reason of what? A. Oxidation.

Q. What causes the oxidation of coal?

A. The oxygen of the air.

Q. Does oxygen of the air combine with the sulphur of the coal, or does it combine with pyrites or other substances in the coal?

A. When there is no heat in the coal the combination is mailing with a carbonaceous matter rather than with pyrites, but the two types of oxidation are common under conditions, so that both types of oxidation are possible at all times.

Q. What are the two different types of oxidation, Professor?

A. The oxidation of the carbonaceous matter is one type; the oxidation of the sulphur in the pyrites is another type.

Q. Which one causes an increase in weight?

(Testimony of S. W. Parr.)

A. Both of them.

Q. That process is very slow, is it not?

A. It depends on temperature and other conditions.

Q. Take a place where there is a normal temperature, like in San Francisco.

A. It is slow in comparison with the changes due to the filling of water but it is difficult to say what you would mean by slow.

Q. Let us take the month of June or July, for a **[1451—1389]** period of 15 days, where the temperature is normal, and there is an entire absence of rain, would the process of oxidation in that case be slow or rapid?

A. The term slow is so relative, I don't know what you mean by slow.

Q. Well, what difference would it make in weight; the amount of loss of weight would determine the speed or the slowness of the oxidation, would it not?

A. Yes, that's very good; I think in those conditions a change in say for that length of time might be covered by a half per cent. There are plenty of illustrations, however, where changes due to oxidation will reach 2 per cent.

Q. Do you say that in the period of 15 days' oxidation would be carried on to such an extent that the weight of coal would be increased one-half of one per cent?

A. I understood your first question to include a period of a month?

Q. No, 15 days I put it. A. 15 days?

(Testimony of S. W. Parr.)

Q. Yes.

A. Well, it might increase a half per cent in that length of time; it might increase one-quarter of one per cent in that length of time.

Q. It might increase one-quarter of one per cent or it might increase one-half of one per cent through oxidation alone?

A. This is possible, entirely possible.

Q. Can you conceive of this condition, Professor, where one of the barges of the Western Fuel Company was loaded with a cargo of coal on May 16th and the entire cargo was discharged by the 21st of May; and the increase amounted to 23½ per cent during that period of time, no **[1452—1390]** rain in the meantime; can you conceive of any case where oxidation would be carried on to such an extent during a normal condition of weather?

A. I could not.

Q. Can you give any possible theory upon which the weight of coal under such circumstances increased during that period of time from May 16th to the 21st of May, the increase being 23½ per cent?

A. Not to natural conditions.

Q. Not to natural conditions?

A. No, certainly not.

Q. Now, during that period of time, the Nanaimo coal such as you know it, would increase in weight to what extent, do you think, naturally through the oxidation process, that is, for a period of six days?

A. Would you confine it to oxidation processes alone, may I ask?

(Testimony of S. W. Parr.)

Q. Or to any natural process which results in the increase in weight in coal, during normal weather and in the absence of rain?

A. And over what length of time?

Q. Six days?

A. I do not think that oxidation processes and humidity processes in the absence of rain would change the weight of coal or increase the weight of coal *possibly* to more than possibly one-half per cent or one per cent.

Q. You said a little while ago that in 15 days, the increase in weight by reason of oxidation would be about one-quarter of one per cent and from that to one-half; now, I bring it down to six days; do you say it would increase in weight in six days one-half of one per cent?

A. But your first question related only to oxidation; you are admitting now the possibility of humidity in addition. [1453—1391]

Q. I am taking now the normal condition in San Francisco, and the absence of any rain.

A. But you didn't so define your first statement, but you do now.

Q. Well, what would be the increase in weight of coal, say like Nanaimo coal, during 15 days of normal weather in San Francisco, and where there is no rain at all, what would be the increase through oxidation or any other cause natural to coal?

A. It would be very difficult to say. There would be a slight increase in the coal.

*   *   *   *   *   *   *   *   *

(Testimony of S. W. Parr.)

Q. Do you think there is any humidity in the atmosphere outside to-day?

A. Certainly there is; 80 per cent is about the average of San Francisco humidity.

Q. All right; we will take San Francisco's humidity at 80 per cent. Assume the humidity is 80 per cent. Assume that the coal is exposed for about 15 days. Assume that the usual oxidation takes place, what would be the increase in weight during that period of time?

A. You have not included any temperature changes in the coal itself.

Q. Take the ordinary temperatures of San Francisco? A. I mean temperature in the coal.

Q. Take the normal condition of the coal.

A. You are assuming the coal at normal condition, and humidity at an average of 80 per cent, and oxidation and humidity processes are going on for a period of 15 days?

Q. As they usually go on, yes.

A. It would have to be given at rather wide limits, possibly from one-half to 1½ per cent.

Q. During 15 days. Then a coal merchant can [1454—1392] make more money by storing his coal than he can by selling it, for six months?

A. That depends on what his margin of profit is.

Q. If coal increased at the rate of 1½ per cent in 15 days, 3 per cent in 30 days, or 6 per cent in 60 days, it would pay him, would it not, to store his coal for six months before he sold any of it?

A. I have not said that that ratio would keep up.

(Testimony of S. W. Parr.)

Q. How long would this terrific rate keep up of $1\frac{1}{2}$ per cent for 15 days or 3 per cent for 30 days?

Mr. McCUTCHEN.—He didn't say 3 per cent in 30 days.

A. This variation in oxidation is a matter that varies from time to time, and there is no possible way of saying what would be the continuation of that condition, whether it would be a proportional change from day to day and from month to month, or not.

Mr. SULLIVAN.—Q. Suppose that kept up for two months, $1\frac{1}{2}$ per cent for 15 days, 3 per cent for 30 days, 6 per cent for 60 days and rain came along to boost it along, how much would the coal increase in weight if one inch of rain fell at the end of 60 days?

A. You have introduced conditions which I have not conceded at all. There is a limit to oxidation property. You cannot oxidize sulphur after there is no sulphur to oxidize. How could you increase the weight after the sulphur is all gone?

Q. As I understand it, the oxidation takes place in the substance of the coal if there is no sulphur there at all, does it not?

A. If there is no sulphur there at all?

Q. Yes; can oxidation take place in the coal in the absence of any sulphur in the coal?

A. Yes, it can. **[1455—1393]**

Q. And oxidation can take place as long as there is any sulphur in the coal left?

A. There cannot in this type we are speaking

(Testimony of S. W. Parr.)

about, which is oxidation by atmosphere under normal conditions, there is a saturation point beyond which the oxidation will not go; and it is absolutely absurd to assume that oxidation continues from month to month and from year to year; that is an absurdity on the face of it.

Q. How long does oxidation continue, Professor?

A. At normal temperature?

Q. Yes, at normal temperature.

A. I don't know; it varies.

Q. About how long does it continue?

A. It grows less and less until it has completed the saturation for which it has set out and then it stops; the longer it goes the less it is in amount.

Q. How long does it take in normal weather with this Nanaimo coal for oxidation to reach that stage?

A. That is impossible to say. No one has followed the case out.

Q. What is your best judgment?

A. I would not attempt to answer.

Q. Can you fix the period beyond which it won't go, beyond which oxidation won't continue?

A. I should say that so far as human experience goes you could say in 100 years perhaps it would be complete.

Q. Then oxidation might continue 100 years; is that the fact?

A. I didn't say that; I said that if human experience would go to that extent, but the vanishing point, it is **[1456—1394]** at a lower and lower point;

(Testimony of S. W. Parr.)

there comes a time when it is so small it would be impossible to measure.

Q. The vanishing point might be 100 years hence, and the oxidation becomes less and less as you reach that vanishing point; is that what you want to say?

A. The oxidation becomes less and less.

Q. It continues for at least 60 days, does it not?

A. Not in amount. It is greater the first 6 or 8 days than it is the last 6 or 8 days.

Q. It will continue at least 60 days as a rule, will it not, to an appreciable extent?

A. Probably to an appreciable extent, but not in the ratio that it started out; no one can tell what the relative increase in oxidation at any one time is compared to another time.

Q. Take the case of a single lot of coal stored in the yards, 100 tons or 1,000 tons, oxidation will continue in that coal from the time it is deposited in the yard, for at least 60 days, will it not?

A. I presume it will.

Q. In that coal what would be the increase in the weight of the coal under normal conditions, assuming the humidity to be 80 per cent, and assuming the usual oxidation takes place?

A. I think I have given as an average amount of increase due to oxidation at about a half per cent.

Q. For the whole sixty days. What would be the increase in the weight of the coal for those 60 days?

A. I think a half a per cent would be a conservative estimate if you included the humidity. You are talking now about oxidation.

(Testimony of S. W. Parr.)

Q. I am talking about the ordinary humidity of 80 per cent, as you call it, for 60 days. You say the ordinary **[1457—1395]** increase would be one-half of one per cent?

A. Including humidity it might be more than that.

Q. That is, including the ordinary humidity?

A. Humidity and oxidation of all sorts.

Q. How much would it be? How much would it be in your opinion? A. For a period of 60 days?

Q. Yes.

A. I think the range might be perhaps from ½ to 1½ per cent.

Q. From ½ to 1½ per cent; for 30 days what would it be? A. I don't know.

Q. A little while ago you said it would be from ½ to 1½ per cent in 15 days; is there any difference in the extent of the oxidation which takes place during the period of 15 days and the amount of oxidation which takes place during a period of 60 days?

A. There might be. It is not a constant and you cannot say by one period of time what it will be for a corresponding period of time later on.

Q. It is all a mere matter of conjecture, is it not, mere guesswork?

A. Under the conditions which you prescribe it would have to be guesswork.

Q. Yes, that is what I thought. Take this case of the barge "Comanche" which took on her cargo on the 16th of May and discharged it fully on the 21st of May, increasing its weight 23½ per cent; what would be the normal increase under ordinary condi-

(Testimony of S. W. Parr.)

tions which you would allow for that cargo due to oxidation and ordinary humidity in the air, for those six days, and due to any other causes naturally affecting coal, excepting rain?

A. I think it might occur within a range of possibly one half of 1½ per cent **[1458—1396]** increase due to those conditions.

Q. Then this increase of weight for the period of six days would be substantially the same as the increase in weight through oxidation for a period of 60 days; is that a fact?

A. It might be and it might not.

Q. So, then, as a matter of fact, it does not make any difference as far as the increase in weight is concerned whether the cargo is subjected to oxidation for 6 days or for 60 days?

A. That does not follow. That is not a conclusion that can be drawn. I said in my first period of time, we will say for 6 days, that the range might be from ½ to 1½ per cent. Suppose there was an increase for those 6 days, an increase in weight of ½ per cent; suppose in the last period of time the increase has gone on until it reaches 1½ per cent, what is there inconsistent in my statement?

Q. Let me see if we understand you. You said you assumed the process of oxidation to go on until in 60 days the increase in weight would range from ½ of one per cent to 1½ per cent; is that correct?

A. I said the range might be within those limits.

Q. Now, you have also in response to my questions said that in the case of the "Comanche" the increase

(Testimony of S. W. Parr.)

in weight under oxidation, with the ordinary humidity, for the period of 6 days, would be from ½ to 1½ per cent? A. It might be from ½ to 1½ per cent.

Q. So that logically it necessarily follows, does it not, that there is no practical difference in the **[1459—1397]** amount of increase in weight between a cargo subject to this exposure for 6 days and a cargo subject to exposure for 60 days?

A. That does not follow at all.

Q. That is about as close as you can come to it, is it not, Professor, as to the increase in weight for any particular period of time, 6 days or 60 days?

A. With the very vague conditions under which we are discussing the case I should say so.

Q. Is there any point of limit beyond which the weight is increased by the absorption of water rain?

A. Will you please ask that again?

Q. What percentage is the weight of ordinary coal, Nanaimo coal, to what extent can it be increased, that is, the maximum extent by the addition of water?

Mr. OLNEY.—You mean to the saturation point.

Mr. SULLIVAN.—Yes, to the saturation point.

A. It depends entirely on the percentage of the fine material in the coal.

Q. Then the average Nanaimo coal discharged from a barge—

A. (Intg.) There is a limit beyond which the increase due to moisture would not be possible, giving the coal an average opportunity to drain.

Q. What is that limit? In other words, what is the point of saturation?

(Testimony of S. W. Parr.)

A. Assuming that coal has we will say 25 per cent of fine material or slack, as it is called, I think the saturation limit would probably be 8 or 9 per cent. [1460—1398]

Q. That is the maximum extent of the increase by the addition of moisture, as I understand you.

A. Such moisture as it will retain.

Q. That is, such moisture as it can retain.

A. May I add one other condition, that this moisture addition covers a comparatively short period of time.

Q. You say it covers a period of 7 or 8 days, or two weeks, say? A. Yes.

Q. Even if the water is added to the coal by means of a hose, a Pacific Mail Steamship Company hose, or by reason of a downpour of rain. A. Yes.

Q. Now, I direct your attention to the "Nanaimo," which took aboard a certain quantity of coal on the 3d day of February, 1906, and completed the discharge of that coal on the 10th day of February, 1906, and the increase in weight was 32½ per cent. * * * Assume the whole barge full of coal.

A. I should say that an increase in weight to the extent that you claim is beyond what could be normally expected from the addition of water alone.

Q. Is it beyond your comprehension, or the comprehension of any scientist or any chemist, that that increase could have been due to oxidation or any other cause natural to coal which causes an increase?

A. It is.

Q. In other words, there must have been some-

(Testimony of S. W. Parr.)

thing peculiar done in the weighing of that coal in order to increase it to that extent; is not that so?

A. I would not attempt to say what was done to it, but it was not due to natural causes.

Q. It was not due to natural causes; there was something wrong about it?

A. I do not know; that **[1461—1399]** might be explained on other bases.

Q. Now, take the case of the "Theobold" which loaded a cargo of coal, average coal, on the 29th day of June, and fully discharged the cargo on the 6th day of July, 1906, and no rain prevailing at that time; what ought to have been the amount of increase of weight in that coal during that period of time, 6 days in July and two days in June, 8 days; give me a maximum amount of increase that ought to be found in that cargo of coal?

A. I could not give you a definite figure; it would have to be within limits again, as I did in the other case.

Q. Would it be from ½ of one per cent to 1½ per cent, as in the case of the 6 day cargo?

A. I think it would be within those limits.

Mr. SULLIVAN.—Q. Take the case of the "Ludlow," which took on a cargo of coal on the 27th of June and discharged it fully on the 16th of July, about 20 days all told; now, during that period of time, I suppose the natural increase due to oxidation and other causes ought not to have been any more than from ½ to 1½ per cent?

(Testimony of S. W. Parr.)

A. If it didn't include rainfall, that would be the limit, I should say.

Q. And if it did include rainfall it would be not any more than 8 or 9 per cent as a maximum; is that so? A. Yes.

Q. What quantity of rain would have to fall upon a cargo of coal say of 100 tons, or 1,000 tons, say, in order to have the point of saturation reached?

A. If it were coal which had approximately 25 per cent of fines, an amount of moisture which would raise the weight to 8 or 9 per cent, I would say would reach the limit of saturation, whether it was a large amount or a small amount.

(Witness continuing.) The quantity of rain would depend entirely upon the area to which the coal was exposed. In the case of these bunkers that I have seen, or in the yard, it would be necessary to take the square feet of area in a given bunker; it would not make so much difference with regard to the quantity of coal. **[1462—1400]**

I know that the coal at Folsom Street remains in the bunkers a comparatively short time. You cannot see the oxidation of the substance of coal, nor is the oxidation of the sulphur in the coal generally visible. To a very slight extent, that is to say, in proportion to the weight that is increased, and the heat value thereby decreased, oxidation affects the commercial value of the coal. You cannot increase the weight of coal by noncombustile material without decreasing the heat per given unit of weight.

Q. Now, Professor, where spontaneous combus-

(Testimony of S. W. Parr.)

tion arises and no water is added to the coal, does the coal increase in weight or diminish in weight?

A. That would depend upon whether the oxidation was carried to an extent where fire and smoke resulted; if there was no escaping of gases and volatile material, the oxidation would result in an increase in weight, even though there would be no water added to the coal.

Q. And, of course, the loss by reason of oxidation in case of actual fire would be in proportion to the amount of smoke and fire. A. Yes, sir.

Q. You say the Australian coal is peculiarly subject to oxidation? A. It is.

Q. Is that a bituminous coal also? A. It is.

Q. In transit from Australia to California, does that coal increase in weight, or diminish in weight, as a rule?

A. There are two processes going on, one of oxidation, and one of evaporation, I would suppose because of the region through which the coal passes. [**1463—1401**] The chances are, I would say, that as between the two, an average or resultant effect would be that there would be a slight loss in weight.

Q. That is, the loss of evaporation would be greater than the increase of weight by oxidation?

A. I think it might well be so. That is a point on which I have no data.

Q. You understand, of course, the coal crosses the equator once. A. Yes, sir.

Q. Does heat increase the oxidation?

A. It does.

(Testimony of S. W. Parr.)

Q. The greater the heat the greater amount of oxidation?

A. The more rapid is the oxidation, and presumably the greater amount.

Q. The more rapid the oxidation the greater the increase in weight? A. Yes.

Q. It takes about two months, does it not, to make the trip from Australia to California—60 days?

A. No, it does not, I think about 33 to 35 days, ordinarily.

Q. And during that period of time, what, in your opinion, is the increase in weight by oxidation, making no allowance for the loss in weight by evaporation?

A. I think perhaps it might be covered by the same limits we gave before, one-half to one and a half per cent.

Q. One-half to one and a half per cent, although the coal is in transit for quite awhile through a very hot portion of the globe. A. Yes.

Q. And that would be more than offset, you believe, by reason of the evaporation.

A. That would depend **[1464—1402]** on individual cargoes; it would be impossible to make a general statement covering a case of that sort.

Q. Take an ordinary cargo, under ordinary conditions prevailing in Australia, and from Australia to California.

A. It would be a very difficult matter to say whether the amount of oxidation exceeded or was less than the amount of evaporation.

(Testimony of S. W. Parr.)

Q. Now, we will take the case of a shipment of coal from Nanaimo to San Francisco; I understand it takes about four days for a cargo to reach San Francisco; from the time the coal is put upon the ship, until it is discharged from the ship at the Folsom Street bunkers, the process of oxidation continues right along, does it not? A. It probably does.

Q. And more rapidly than at a later stage?

A. Well, the fresher the coal I would say possibly yes, but that is also offset by the fact that the fresher the coal the more it loses in weight due to the evaporation of the water, so that it is impossible to make a general statement covering the relative amount of those two processes.

Q. If the cargo is covered over, if the hatches are closed, there is very little evaporation, is there not?

A. Very little evaporation.

Q. And in that case, there would be an increase in weight, rather than a decrease in weight, would there not, during that period of time?

A. I would not be willing to say that.

Q. Well, what is your opinion, Professor, from your knowledge of coal?

A. It is very difficult to formulate an opinion where two processes are going (on), which are substantially equal in amount and say which will overbalance **[1465—1403]** the other. I do not see how it would be possible to make an exact statement which would be required to cover the case in hand.

Q. In the case of Australian coal, which passes across the equator, you would say that the increase by

(Testimony of S. W. Parr.)
oxidation is offset by the decrease by evaporation during the period of sixty days.

A. I didn't say that.

Q. I mean 33 days.

A. I said I would not like to make a statement as to which exceeded the other.

Q. Didn't you say one would about offset the other?

A. I said it would be a difficult matter to say.

Q. Well, what is your opinion?

A. Under some conditions, it would be a very easy matter for the oxidation to surpass the amount of loss due to evaporation; under other conditions, the evaporation might be greater. The two are nearly equal in amount, and it is difficult to say which would exceed.

Q. Wouldn't there be more evaporation coming across the equator than there would be coming down from Nanaimo to San Francisco?

A. There would be more evaporation in all probability, because the vessels which carry the Australian coal are especially arranged for ventilation, because of the danger of oxidation, so that there would be more loss by evaporation in Australian coals than in the case of Nanaimo coals.

Q. Now, don't you think, under ordinary conditions, that would be the case?

A. That there would be more water lost in one case than in the other?

Q. Yes. A. I do.

Q. So, take the ordinary trip from Nanaimo to

(Testimony of S. W. Parr.)

San Francisco, in an ordinary collier, with hatches closed, the [1466—1404] evaporation is comparatively slight, is it not? A. Comparatively slight.

Q. Don't you believe as a scientist and an expert on coal that the evaporation in that case would not create a loss equal to the increase created by oxidation? A. That might be possible.

Q. Is it not true, then, Professor, that the increase in weight due to oxidation would more than offset the loss in weight due to evaporation?

Mr. OLNEY.—You are referring to a Nanaimo cargo?

Mr. SULLIVAN.—Yes, I am referring to Nanaimo cargoes.

A. It might be possible for the oxidation to be greater than the evaporation, but whether that is a case which would generally be true, or not, would be impossible to say, it seems to me.

Q. When you were subpoenaed as a witness—I mean before you were employed as an expert in this case, did you not understand that there were two very important questions to come before the court and jury, one was a question relating to shortages, and another one relating to overages?

A. I had a general understanding only.

Q. You knew also that the Government claimed, did you not, that the output of the cargoes in San Francisco after arrival, was in a great many cases much less than the amount of the intake at Nanaimo, the point of exportation?

(Testimony of S. W. Parr.)

A. I do not recall that I knew that statement specifically.

Q. Well, you knew that before you were examined as a witness, did you not?

A. Will you repeat the question again?

Q. I will put this question to you now. Did you [1467—1405] not know, before you were examined as a witness in this case, did you not know before yesterday that it was claimed by the Government in this case that the output of the cargoes in San Francisco were always or in a great many cases less than the amount of cargo taken in at Nanaimo?

A. I think I knew that that was the claim.

Q. And didn't you go over that particular phase of the case with counsel for the defense many times, or several times?

A. Probably we did; I do not recall specifically with regard to that point.

Q. What is that?

A. I do not recall specifically with regard to that point, that we went over it.

Q. Didn't you go over it at all with them?

A. You mean the matter of changes between Nanaimo and this point.

Q. Yes, whether the coal increased or diminished in quantity on the trip from Nanaimo to San Francisco.

A. I think not. I do not recall that we discussed that point especially.

Q. Didn't you give it as your opinion that the coal

(Testimony of S. W. Parr.)

increased in weight in transit from Nanaimo to San Francisco?

A. I do not recall that I gave that as my opinion.

Q. Will you say that you did not give that as your opinion?

A. I do not know that I have expressed that as my opinion, no.

Q. Well, is not that, in fact your opinion, that the coal increased in weight in transit from Nanaimo to San Francisco, instead of diminishing in weight?

A. That might be possible; I do not know that I am ready to say that. I have refused to say positively whether it would be an increase **[1468—1406]** or a decrease, because—

Q. (Intg.) As an expert, have you not at the present moment an opinion upon this question, as to whether coal, under ordinary circumstances, increases or diminishes in weight, in transit from Nanaimo to San Francisco?

A. I would have to figure a little on the proposition. I have testified that the tendency of all coal, upon being broken out from the seam is to lose moisture, and that the tendency of coal is also to absorb oxygen; now, whether these two processes would result in an ultimate loss or increase in weight over a short period of time is a very difficult question to say.

Q. You have testified, have you not, to the amount of increase in 2,000,000 tons of coal during the period of some six or seven years? A. I have.

Q. Where the coal was turning all the time, and

(Testimony of S. W. Parr.)

it is physically impossible to give a correct estimate; you have testified to that, have you not?

A. I have not testified that it was a physical impossibility to give a correct estimate.

Q. Well, don't you know it is a physical impossibility to give a correct estimate by reason of the fact that the coal is shifting all the time, and changing all the time, and has been shifting and changing, every day, every week, every month during the last six or seven years?

A. Not at all. It is a comparatively simple proposition.

Q. Is it not the same proposition for you now to determine as an expert, whether coal increases in weight or loses in weight in three or four days in transit from Nanaimo **[1469—1407]** to San Francisco?

A. In matters of change involving, we will say, one half per cent in amount, where it may be one-half per cent plus or one-half per cent minus, or fractions in the hundredths of a per cent, it is not an easy thing to say.

Q. Don't you know that if you said the coal increased in weight from Nanaimo to San Francisco in transit, that that answer would be hurtful to the defense?

A. Why, it might or might not; I don't see that it would.

Q. Don't you know that on account of the issues involved in this case, the answer would be hurtful to the defense?

(Testimony of S. W. Parr.)

A. I don't think the matters involved in the transit of coal are of enough moment to make very much difference either way.

Q. Don't you know that the Government in this case claims that the Western Fuel Company did not properly weigh the coal that came from Nanaimo to San Francisco, and that the shortages amounted to a very large sum?

A. I understand that is the claim.

Q. Now, you have testified as an expert that coal increases in five or six days by reason of oxidation from ½ to 1½ per cent in weight, That is not an infinitesimal fraction; you do know, from your study, the amount of evaporation of coal under certain conditions, do you not? A. I do.

Q. As a scientist, you know that in a given time, and under given conditions, there will be a certain amount of evaporation from a certain quantity of coal, do you not?

A. Under certain conditions. **[1470—1408]**

Q. Now, will you say that you are unable to tell this jury the amount of loss of coal resulting from evaporation in an ordinary cargo of coal, say 3,000 or 4,000 tons, from Nanaimo to San Francisco, the voyage taking four days?

A. I said the difference between the plus and the minus would be small, and it is difficult to say; that is equivalent to saying that the two very nearly neutralize each other. Now, if you want to call it on the plus side, or on the minus side, you would be as able to do that as I would.

(Testimony of S. W. Parr.)

Q. Will you testify right now that in your opinion there is no loss at all of coal, or practically no loss of coal at all—

The COURT.—You mean in weight.

Mr. SULLIVAN.—(Continuing.) In weight, in transit from Nanaimo, the point of exportation, to San Francisco, the point of delivery.

A. The difference in weight due to the two causes between Nanaimo and San Francisco would be very slight, either way.

Q. Is it not your opinion that there is practically no loss, that the weight of the coal in San Francisco, upon arrival, is practically the same as it was at the time of exportation?

A. I would not say there was no loss; I would say that within the limits it may be a small amount plus or a small amount minus.

Q. Don't you mean by that that there is practically no loss at all?

A. It would be very small either way.

Q. What do you mean by small either way?

A. Well, I would say perhaps ¼ of a per cent variation, plus or minus. **[1471—1409]**

Q. That is, it might increase ¼ of a per cent in weight, or it might diminish ¼ of a per cent in weight.

A. The ultimate resultant of the two processes might be that amount, either way.

Q. Do you believe, Professor, that under normal conditions, in transit from Nanaimo to San Francisco, the shortage could be equal to 2 per cent?

(Testimony of S. W. Parr.)

A. I do not think it could, due to moisture changes.

Q. Or to any other change?

A. I do not think to normal, natural causes, such as would be chemical in character.

Q. Do you think the change could exceed ¼ of 1 per cent under normal conditions, that is, that the shortage could amount to more than ¼ of 1 per cent?

A. I would say that under normal conditions the limits would be perhaps a quarter to a half; it could hardly exceed that.

Q. Now, you are going to the half; under no circumstances, in your opinion, could it exceed one-half of 1 per cent?

A. I should think that would be the limit.

Q. How often have you been at the bunkers during the last month?

A. I probably have been there half of the days that have intervened.

Q. I want to go back to this other question for a moment: Assuming that the cargo is shipped in winter-time, when we have rains along the coast, is it not probable that the coal would increase in weight from Nanaimo to San Francisco, rather than diminish?

A. You are assuming now that the cargoes are covered. **[1472—1410]**

Q. Assuming that the cargoes are covered.

A. And that there is no accession of rain.

Q. And there is no accession of rain to the coal.

A. I can hardly see any reason why the cargoes

(Testimony of S. W. Parr.)

would increase in weight during the winter months.

Q. Under no conceivable circumstances would you say thae either in the winter-time or in the summer-time, the hatches being closed, the loss could be any more than ½ of 1 per cent?

A. I think that would be a fair limit.

(Witness continuing.) I made my experiments with the coal in the bunkers for the purpose of determining the weights, and for the purpose of finding out what quantity of coal would be contained in a bucket—fines and coarse—on January 20th. We weighed only two tubs on the "Theobold," No 2 and No. 3. We didn't fill the four tubs. The tare of No. 2 was 590 pounds; the tare of No. 3 was 503 pounds. We didn't weigh any tubs on the "Comanche" with respect to fines and coarse. We weighed tubs No. 1 and No. 2 on the "Comanche." The tare of tub No. 1 on the "Comanche" was 730; the tare of No. 2 was 600. We only used one tub on the "Nanaimo," tub No. 4 with a tare of 574 pounds. We put three loads on it. If the tub was filled at the mouth to the water line, as they call it, and filled, say, to within one foot of the water line at the rear with coal, ordinary coal, it would not tip at the bunker.

Professor Sommermeier was with me during my experiments and examination last August. He took his own samples and I took mine. He has been with me many times but not always on my visits to the bunkers here in San Francisco, and he has also been with me at the yard. We have been working together as experts **[1473—1411]** for the defense

(Testimony of S. W. Parr.)

and have compared notes and I assume that we practically agree upon the main facts on which we are to testify. The experiments on the weight of tubs at the barges were made by us jointly. However, we have not been generally, though we have been frequently together in our experiments. He has his own line of work and his own conclusions and I have mine. We made out separate reports to the Western Fuel Company. He did not submit his report to me, nor did I submit mine to him before they were respectively rendered to the company, though we gave each other copies of our reports afterward and in most respects we agreed.

Q. In your testimony yesterday, you gave certain figures, you read certain statements from a book which appears to be Bulletin 41, issued by the Department of the Interior **[1474—1411½]** Bureau of Mines; did these figures deal with anthracite coal or with bituminous coal?

A. Sometimes anthracite, sometimes bituminous.

Q. And did you, in your testimony yesterday, give the minimum and the maximum figures for the purpose of showing variation?

A. I did, variations due to moisture and changes in weight.

Q. How is that?

A. Figures which showed the variation in moisture.

Q. These figures here showed the variation in moisture each month, did they not, according to your recollection? A. They did.

(Testimony of S. W. Parr.)

Q. And you took the highest and the lowest in each case?

A. I did not; I took the highest and the lowest in the cases I read; not necessarily the highest and the lowest, but I took what caught my eye as being the highest and the lowest.

Q. Had you gone over this report before?

A. I had.

Q. You had gone over the report before?

A. I had a copy of the report.

Q. And you marked the figures concerning which you intended to testify in this book, did you not?

A. I did.

Q. Now, for instance, in reading from the Bulletin, here, containing a report of an analysis upon coal taken from Pocahontas and New River run of mine, West Virginia, you gave the moisture contents in July as 1.63 and in January—

A. What page are you reading from, please?

Q. Page 39. **[1475—1412]**

Mr. OLNEY.—The witness did not state the dates, Mr. Sullivan.

Mr. SULLIVAN.—No, but he did state the percentage.

Mr. OLNEY.—He was asked to take the large differences.

Mr. ROCHE.—That was the question he has practically testified to.

Mr. SULLIVAN.—(Continuing.) And in January, 1910, moisture content 7.10, do you see that?

A. Yes.

(Testimony of S. W. Parr.)

Q. One is a low figure, and the other is the highest figure, 7.10? A. Yes.

Q. That coal at no time contained as high as 7.10 moisture in that pile from July, 1909, to January, 1910, did it? A. Please repeat that question.

Q. I say, the moisture content of that coal at no time exceeded 7.10 per cent.

A. Presumably not.

Q. That is more than twice the amount of average moisture contained during that entire year, is it not, in that coal?

A. That is quite possible, I have not figured it out.

Q. Now, is it not a fact that the average, which is down below there in the average, is 3.16 per cent of moisture content? A. I have not averaged it.

Q. You will see that the average is printed below there, 3.16? A. Oh, yes, very good.

Q. Less than half the maximum figure of moisture content which you gave yesterday?

A. Very good.

Mr. McCUTCHEN.—The witness was expressly asked to indicate the large differences.

Mr. ROCHE.—There isn't any question about that, **[1476—1413]** Mr. McCutchen, but we want to get before the jury the fact that he did select the maximum figures.

Mr. McCUTCHEN.—He was asked to do that. The witness did not mislead anybody; that is what Mr. Olney asked him to do.

Mr. SULLIVAN.—Q. Take page 37, showing the moisture content of the Sugar Load run of mine, Mil-

(Testimony of S. W. Parr.)

ler bed; you read there the moisture content of coal as 3.90 and 1.42?

A. I did, and I stated just before reading that that I had specifically taken the extremes to answer that question as it was put.

Q. And you gave the average of those two figures yesterday, did you not?

A. No, I didn't give the average.

* * * * * * * * *

Mr. SULLIVAN.—Q. You stated yesterday that the moisture content of coal was in July, 1909, 1.42, and in January, 1910, 3.90; you did not state that the average, though was 1.92, did you?

A. No, I did not.

Q. And 1.92 is less than one-half of the high moisture content of January, 1910, is it not?

A. It is.

Mr. McCUTCHEN.—May I ask, were those averages all obtained by adding together the low moisture content and the high moisture content and dividing the aggregate by two?

Mr. ROCHE.—No, of course not.

Mr. SULLIVAN.—By the number of months.

Mr. McCUTCHEN.—Oh, by the number of months. I thought it was a sample taken in one month, and then taken in another month.

Mr. ROCHE.—There are a great many months here that are far below that average. **[1477—1414]**

Mr SULLIVAN.—Q. What do you mean by the run of the mine, Professor?

A. The coal as it comes up in the cars as prepared by the miner below.

(Testimony of S. W. Parr.)

Q. Does not the run of the mine include all of the coal just as it comes from the mine, the lump coal, the fine coal, and all kinds of coal that is run out of the mine? A. Yes, sir, for the market.

Q. Don't you know that at Nanaimo the coal that is mined there is run over a screen before it comes out? A. It is not.

Q. It is not screened at all?

A. Before it comes out of the mine, it is not screened.

Q. Before it is shipped here and before it is weighed? A. It is.

Q. Where is it run over the screen?

A. At the tipple.

Q. You don't call that the run of the mine? That coal always contains less moisture than the run of the mine coal, does it not?

A. Which coal do you refer to, the screen coal?

Q. The run of the mine is the screen coal, the lump coal, and all kinds of coal you have mined in the mine, itself? A. It is.

Q. This coal at Nanaimo, before it is weighed, is passed over a screen, and only coal of certain sizes is weighed; is not that so?

A. All the coal is weighed, of whatever size.

Q. I mean, the ordinary coal is separated from the fines, is it not, by means of these screens. A. Yes.

Q. There is not as much moisture in this coal which is separated from the screens as there is in the ordinary run of the mine coal, is there, the coal that does not go over the screens? Is there not less

(Testimony of S. W. Parr.)

moisture in coal that **[1478—1415]** is free from the screen than there is in the ordinary run of the mine coal?

A. I do not see how that could be the case, when the coal immediately comes up from the bottom. The coal as it is broken off from the seam has the same moisture throughout the seam.

Q. Then do you say that the screenings weigh the same as the coal that is separated from the screenings? A. A ton of it would weigh the same.

Q. I am not talking about a ton of it, I am talking about the cubic contents of it; I mean to say the coal, itself, proper, is there not more moisture in a ton of screenings than there is in a ton of coal that is separated from the screenings?

A. In a ton of lump?

Q. Yes.

A. The screenings cannot lose the moisture so rapidly as the lump, hence the tendency is for the screenings to run a little higher in moisture.

Q. Let us take the case of the lump coal, separate from the screenings, put a ton of that coal in one place, and take a ton of screenings and put it in another place, won't the screenings contain more moisture immediately after the separation than the lump coal itself?

A. I cannot see why that would be the case, it certainly would contain more moisture a few days after, when the evaporation had had a chance to go on because the evaporation from the lump is easier than it is from the screenings.

(Testimony of S. W. Parr.)

Q. These cases here all indicate the moisture content of the ordinary coal which contains lump and screenings, and in some cases the screenings alone—these figures you gave yesterday, do they not? **[1479—1416]**

Mr. ROCHE.—Take a top table on page 39, Professor.

Mr. SULLIVAN.—Q. Where is the coal delivered to the Government, Professor, is it delivered at the mine, or delivered at the Government station?

A. Delivered at the Government building.

Q. This table here on page 39, which you referred to a little while ago, refers to analyses of coal delivered to the Government under contract; don't these variations here shown upon page 39 show the changes wrought by the weather, by the rain or snow, for instance, in January, 1910, the moisture was 7.10.

A. That would be impossible to state, unless we knew whether it was a car ship in a closed or box-car, or not.

Q. Do these figures here indicate the moisture at the mine, or the moisture at the place of delivery?

A. The moisture at the place of delivery.

Q. If the moisture content in July, 1909, was 1.63, and the moisture content in January, 1910, were 7.10, of necessity it must have been due to the weather, the change in the moisture content—is that not so?

A. Yes, sir, that would be the conclusion.

Q. And then, in February, the moisture content appears to be 4.35, or more than double the amount of the moisture content, 1.63? A. Yes.

(Testimony of S. W. Parr.)

Q. If this coal were screenings, instead of lump coal, the moisture content would be much greater on account of the fineness of the coal. It not that so?

A. That would depend entirely on the weather. The screenings upon leaving the mine would hardly have more than 2 per cent of moisture in them.

Q. How much?

A. They would have the moisture which **[1480—1417]** is characteristic of that particular seam; I do not know what it is.

Q. But the absorbent qualities of screenings are much greater than the absorbent qualities of lump coal?

Mr. OLNEY.—Have you completed your other answer, Professor?

A. I have not. I would say that so far as the tables are concerned, the indications are that the normal moisture of that coal, as it leaves the seam, would be about 1.2 per cent.

Mr. SULLIVAN.—Q. What would you say, Professor?

A. I would say that so far as the table states anything, the indications would be that the moisture content of the coal as it leaves the mine would be about 1.2 per cent.

Q. What table are you referring to now, Professor? A. The same one you have there.

Q. On page 39? A. Yes.

Q. Are you referring to the one above, or to the second table? A. To the middle one.

Q. To the middle one, that is right. And although

(Testimony of S. W. Parr.)

you testified yesterday that the moisture content of coal at a certain time was 7.10, the normal moisture content of the coal is 1.2 at the mine; that is the fact, is it?

A. I say that the indication of the table is to that effect, that is to say, the delivery in October in that table was 1.2; that is the lowest factor of moisture in that particular coal; hence, it is fair to assume that that coal came from the mine to the place of delivery without any climatic changes.

Q. What kind of coal was that?

A. Pocahontas coal, a low moisture coal.

Q. A bituminous coal, or an anthracite coal?

A. **[1481—1418]** Neither; it is a semi-bituminous coal, halfway between anthracite and bituminous.

Q. Now, assume that a ton of screenings were taken out of that mine, with a moisture content as usual, and a ton of lump coal with the moisture content as usual, in January, 1910, after the rainy weather set in, would not the screenings contain much more moisture than the lump coal, itself?

A. If they were subjected to the same amount of rainfall, the fine material would have a higher percentage of moisture.

Q. How much higher?

A. That would depend altogether on the fineness of the division. I will add this further in regard to that point, Pocahontas coal is a coal which is peculiarly liable to break down into fine material.

Q. Then that is an exaggerated case, is it not?

A. Pocahontas coal is all of that type. Pocahon-

(Testimony of S. W. Parr.)

tas coal has about the largest output of any coal in the West Virginia regions.

Q. I mean for the purpose of showing the moisture?

A. That was the purpose, to show the upper limit capacity of coal for moisture. **[1482—1419]** Having taken the four cans of coal from Nanaimo to my laboratory in Illinois, I set to work to analyze the coal within a few days. Those were the only samples of coal that I took from Nanaimo. I did, however, take samples of Wellington or Nanaimo coal from the bunkers in San Francisco. I presume I took as many as a dozen.

Q. How long after taking each sample did you analyze the sample?

A. It would be ordinarily a few days, possibly a week or ten days after taking the sample, during which time the sample was preserved in the same manner with a seal which would obviate any transmission of moisture.

Q. And each sample you analyzed for the purpose of ascertaining the moisture content?

A. That was part of the analytical work in each case.

Q. Did you analyze any of the samples which you took from Nanaimo, or which you took from the yards and bunkers in San Francisco for the purpose of ascertaining the increase in weight by oxidation?

A. Not with reference to the specific time in which they were in storage but as to the ability or rapidity of the samples to absorb oxygen.

(Testimony of S. W. Parr.)

Q. Did you at any time from the date of your original employment up to the present time make any test of coal for the purpose of determining the increase in weight resulting from oxidation?

A. I did.

Q. When?

A. On the samples which were taken at Nanaimo.

Q. Well, I understand you to say that you analyzed those samples with 2 or 3 days after the samples **[1483—1420]** reached your laboratory; is that so?

A. I did.

Q. How then could you make the tests *then* of that coal inside of 3 or 4 days after receiving the samples for the purpose of ascertaining the increase in weight resulting from oxidation?

A. By reason of the fact that the tests on the coal could be made after that time as well as before, with regard to the avidity of the coals for oxidation under the conditions that they were subject to. I made a report on these samples.

Q. That report does not show an increase in weight resulting from oxidation of the samples, does it?

A. It does not.

Q. Have you any report or did you ever make any report since you entered originally upon your employment by the Western Fuel Company showing an increase in weight or the percentage of increase in weight of any sample which was taken from either the Nanaimo mines or from the yards or bunkers in San Francisco?

A. There was no test as to the amount of oxidation

(Testimony of S. W. Parr.)

during the time in which the sample was exposed.

Q. No. You understood, did you not, when you were employed, that you were to testify to the increase in weight of the coal during a given period?

A. Yes.

Q. Resulting from oxidation? A. I did.

Q. Why then, if you knew that to be a fact, if you knew that one of the purposes of your employment was to ascertain the increase in weight resulting from oxidation, didn't you take a sample from the coal at Nanaimo and expose that to the air for a period of time, say for one **[1484—1421]** week to 3 or 4 or 5 or 6 months for the purpose of ascertaining the increase in weight resulting from oxidation during that period of time?

A. I exposed such a sample to such tests as would indicate to me the activity of that coal for oxidation.

Mr. SULLIVAN.—Q. Now, you have testified oxidation increases the weight of coal from ½ of one per cent to 1½% in six days or in 60 days. Now, to prove that fact, why did you not take samples of the coal from Nanaimo, or samples of the coal from the yards or bunkers in San Francisco and expose those samples for a given period of time, say one week or 30 or 60 days, so that you could come before this jury and tell this jury that from an actual experiment you ascertained that the weight of the coal was increased a certain percentage during that period of time by reason of oxidation?

Mr. McCUTCHEN.—I think counsel has inadvertently misstated the witness. My recollection is

(Testimony of S. W. Parr.)

that the witness stated from one-half to 1½ per cent would be due to oxidation and to humidity.

Mr. SULLIVAN.—No, he did not.

The WITNESS.—Then I wish to correct the statement because, as I understood it, it included other things besides oxidation.

Q. The question that was put to you yesterday and the answer that was given clearly established the fact that you said that the increase in weight from oxidation alone ranged from ½ of one per cent to 1½ per cent in six days or in 60 days.

A. And then as the question proceeded, I insisted **[1485—1422]** that the humidity be a part of that change.

Q. Will you point that out in your testimony. I have the testimony here. Have you discussed this question of the increase in weight, within a given period of time, the weight increasing by reason of oxidation, with counsel on the other side since the adjournment yesterday afternoon? A. I have not.

Q. Have you in any way changed your views as to the total amount of increase in weight by reason of oxidation within a given period of time?

A. I have not.

Q. In your opinion, then, is that your testimony as was expressed yesterday?

A. If you will take my expression of yesterday in the manner in which I intended it—if I have given a different expression to what you think I have, it should be changed.

Q. Answer this question: if the weight of the coal

(Testimony of S. W. Parr.)

increased to such an extent as you have testified in your examination here as the result of oxidation, why didn't you take samples from Nanaimo or from the bunkers or yards in San Francisco and submit the samples to exposure for a certain length of time for the purpose of testifying to the actual increase in weight by reason of oxidation?

A. Because there are other tests which would apply to the avidity of the coal or activity of the coal in that connection, which would satisfy me.

Q. Now, isn't the only practical and sensible test for the purpose of ascertaining the increase in weight resulting from oxidation to take a sample of coal and expose it for a given period of time to the air?

A. It does not seem to me so. **[1486—1423]**

Q. It does not seem to you so?

A. No, it does not.

Q. Do you mean to say that you could take a piece of coal or a sample of coal, analyze it one day after it has come from a mine and testify to a jury as a result of that analysis how much oxidation would take place in that coal after six months' exposure?

A. I think that one day would be too small an amount of time.

Q. Do you suppose you could do it after 4 days or 10 days, make an analysis of the coal for the purpose of determining the moisture content and the other elementary content,—do you mean to say that after an examination 10 days from the time the coal was taken from the mine you can determine the amount or the percentage of the increase in weight

(Testimony of S. W. Parr.)

that will result from oxidation in a period of six months?

A. By tests such as I have applied and such as you read in your reference yesterday to a publication of my own, in which there was shown to be an increase in heat value, that was entirely proper for me, understanding as I do what that means, to judge from what is the behavior of the coal with regard to oxidation.

Q. This is a book of your own, isn't it, that you have just referred to and which I showed you yesterday? A. Yes.

Q. Do you know of any other works that have been published showing the effect of oxidation upon coal?

A. I do.

Q. Besides the works you have written?

A. I do.

Q. Do you know of any other works referring to the change or increase in weight resulting from oxidation? A. I do.

Q. Mention them please?

A. In a paper published by Dr. Porter of the United States Bureau of Mines; **[1487—1424]** in the Engineering Mining Journal, a journal in industrial chemistry, I cannot give the date—

* * * * * * * *

Mr. SULLIVAN.—Q. Do you know of any other works?

A. There is a large amount of work published by the Canadian Bureau of Mines which shows oxidation beyond any facts which I have given to you. I

(Testimony of S. W. Parr.)

have that book in the city, I do not have it here this morning.

Q. Are screenings more liable to oxidation than the average coal?

A. As a general proposition, the more finely divided the coal the more rapidly the oxidation.

Q. And the greater the increase in weight?

A. That would be the result.

Q. Now do you remember these experiments conducted by you and referred to in this book which I hold in my hand, written by yourself? A. Yes.

Q. I direct your attention to page 18, Table 3, change in weight of coal exposed to the air. Screenings from Williamson County, Illinois, in an open box on a roof, 311.8 lbs. Weighed December 15, 1908; reweighed June 17, 1909, or about six months afterward, weight 307 lbs. Loss, 4.8 lbs. a decrease in weight of 1.34%. That is correct, isn't it?

A. Yes.

Q. So that according to your own experiments, conducted by you and referred to in this book, there was an open box of coal weighing 311.8 lbs. exposed for about six months upon a roof and at the end of that time instead of there being an increase in weight there was a loss of weight equivalent to 1.54% of coal; isn't that so? **[1488—1425]**

A. That result must be taken in conjuction with other results.

Q. Will you answer that yes or no?

A. I will answer that as I have answered it.

Q. Doesn't that table there show that instead of

(Testimony of S. W. Parr.)

there being an increase resulting from oxidation, there was a decrease of 1.54% during that period of time in the weight of the coal?

A. The results show that *there* must be taken in conjunction with the results which were referred to yesterday.

Q. I am taking the results as depicted in this table alone. Does it not appear upon this table that upon the expiration of six months exposure instead of there being an increase resulting from oxidation there was a decrease in the coal, equivalent to 1.54%. Can you answer that question yes or no?

A. I will not answer it yes or no.

Q. Leave the record stand as it is. You also at the same time conducted an experiment with another box of coal taken from Sangamon County, Illinois; that box contained 227.7 lbs. on December 15, 1908; on June 17, 1909, it was weighed again and it weighed 224.4 lbs.; there was a loss of 3.3 lbs. There was a total loss in percentage during that period of time of six months, or seven months, rather, of 1.45%, according to this table; that is so, isn't it?

A. I will answer that in the same way; it cannot be interpreted without the accompanying facts that go with the table.

Q. Where were these experiments made?

A. At the University of Illinois. **[1489—1426]**

Q. What was the temperature during the summer months there in the year 1908 and 1909?

A. The ordinary summer temperature of that region?

(Testimony of S. W. Parr.)

Mr. McCUTCHEN.—I think it is proper to suggest to the witness if he desired to make an explanation he has a right to do so.

Mr. SULLIVAN.—As soon as I get through he can explain all he wants to.

Mr. McCUTCHEN.—Oh, no; he has the right to make the explanation in connection with his answers to these questions. He said that that could not be interpreted as you interpret it, and it should be interpreted with reference to the table which accompanies it. I think the witness has a right to explain.

Mr. SULLIVAN.—Q. Was there any fall of moisture in December and January and February, 1908, when this coal was exposed upon the roof?

A. In all probability.

Q. And that moisture did not result in an increase in the weight of the coal, did it—the moisture and the oxidations together didn't increase the weight of the coal during the winter months of 1908 and 1909—that is a fact, according to this table, is it not?

A. It is not. That table has nothing to do with that feature of the case whatever.

Q. Well, we will take the other case of a box of coal upon the roof, the coal being taken from Vermillion County, Illinois, November 11, 1908, according to your experiments and your tables, it weighed 289 lbs., on that date, didn't it? A. It did.

Q. And on June 17, 1909, it was weighed again, **[1490—1427]** was it not? A. It was.

Q. It weighed 288.9 lbs., didn't it?

A. I presume it did, according to the table.

(Testimony of S. W. Parr.)

Q. According to the table it did. This table was prepared by you, was it not? A. It was.

Q. And there was a loss there, was there not, of three-hundredths of one per cent?

A. It is so indicated there.

Q. So that in these three samples it appears from the experiments conducted by you in Illinois in 1909, after an exposure of six or seven months in an open box upon a roof of a building, instead of being an increase resulting from oxidation or from any other cause there was a decrease in weight in each case; that is a fact, isn't it?

A. I will not answer that question in the abstract.

Q. Answer it in the concrete then.

A. There are other circumstances that enter into the case, because it would not be fair to the situation, to the results rather, to say that there was an actual decrease.

Q. Well, you made this report, did you not, for the purpose of circulating it throughout the country at that time, to inform those who deal in coal of the qualities of coal and the effect of weathering of coal, didn't you?

A. The primary purpose of the publication was to show the amount of decrease in each value of coals exposed to the weather for different periods of time, and coals submerged under water. The primary purpose was to **[1491—1428]** determine the heat value.

Q. Well, the primary purpose was to determine the heat value. Was there not another purpose also, to notify the world, according to your experiments

(Testimony of S. W. Parr.)

and observations what the effect of weathering upon coal was? A. It was.

Q. The title of the books is "Weathering of Coal"?

A. Yes.

Q. Does not the weathering of coal include the effect of moisture upon coal, the effect of oxygen upon coal likewise? A. It does.

Q. This purports to be a clear and explicit statement of the effect of moisture upon coal, and the effect of oxidation of coal.

Mr. OLNEY.—You mean the tables or the book?

Mr. SULLIVAN.—The book itself.

Q. You have heard of other cases, have you not, where experts have gathered coal from different parts of the country for the purpose of ascertaining the effect of exposure upon the weight of coal? Out there, I will direct your attention to W. A. Powers; do you know W. A. Powers, Chief Chemist of the Santa Fe Railroad Company in 1907?

A. I ought to. He is one of my students and graduates.

Q. He has imbibed your ideas and acquired your practice of making examinations, hasn't he?

A. I presume he did.

Q. I will direct your attention to what you say about various experiments in this book, page 9: "W. A. Powers, chief chemist of the Santa Fe Railroad, **[1492—1429]** in 1907, carried out an investigation of the weathering losses of the coals used on that road. These coals covered a wide range of country, samples being tested from Illinois, Missouri, Kan-

(Testimony of S. W. Parr.)

sas, Colorado and New Mexico. One hundred pound lots of coal were stored in the open air and under water for a period of seven months. The coal stored under water is said to have lost from .26 per cent to 5.92 per cent in weight, and from .56 per cent to 8.75 per cent in calorific value. The coal stored in the open air lost in weight .60 per cent to 4.78 per cent and 1.10 per cent to 9.40 per cent in calorific value."

You understood from that, that Mr. Powers had selected coal from different states in 100-lb lots, put some lots under water and exposed other lots to the air and that during the period of seven months, instead of there being an increase in weight resulting from oxidation or any other cause, there was a decrease in each case or there was a decrease in the case of the lots exposed to the open air from .60 per cent to 4.78 per cent. That is correct, isn't it?

A. It cannot be stated in those terms without the accompanying result?

Q. I am stating it now in the very terms you have used in this book. Are these terms correct?

A. When they are all taken into consideration they are.

Q. Well, now, is oxidation something peculiar to Nanaimo coal or peculiar to this Wellington coal that you have seen, or is it something that occurs in all **[1493—1430]** coals? A. It occurs in all coals.

Q. It occurs in all coals?

Q. Now, what has become of the oxygen, where was the oxygen at that time; was it taking a vacation while this lot of coal was exposed to the elements

(Testimony of S. W. Parr.)

during that period of 7 months—was it working at all during that period of time?

A. It certainly was.

Q. Now, if it was working during that period oxidizing the coal why according to your theory was there a decrease in the weight of coal instead of an increase?

A. There are a good many elements that enter into the case. In the first place because these samples were put in boxes, small in amount, and on top of roofs, and the element of oxidation should be taken into account with the evaporation and all the other constituents that are present.

Q. Well, now, won't the oxidation in a hundred-lb. lot be proportionately greater than the oxidation in a thousand? A. You would have other—

Q. (Intg.) Answer the question yes or no.

A. I won't answer it yes or no.

Q. Won't the oxidation in a hundred-lb. lot be proportionately greater than the oxidation in a thousand? A. It will.

Q. So then, the better way of ascertaining the effect, the increasing effect of oxygen upon coal is to submit the oxygen to small lots of coal; isn't that a fact? **[1494—1431]**

A. That would be a fact if the other constituents and the other conditions under which it operates would remain the same.

Q. Well, you have testified that these 2,000,000 tons of coal increased to 2,062,000 tons by reason of oxidation here where thousands and thousands of

(Testimony of S. W. Parr.)

tons were not subject to oxidation in hundred-lb. lots—

Mr. OLNEY.—(Intg.) The witness has never testified to anything of that sort.

Mr. SULLIVAN.—I will stand upon the record.

Q. How many of these books were published?

A. What was the edition?

Q. This is Bulletin No. 38 Series of 1909, by S. W. Parr and W. F. Wheeler?

A. I don't know the number published.

Q. Did you give your counsel in this case copies of this pamphlet?

A. I think he has a copy; I am not certain about it.

Q. You had, however, this copy, while they were questioning you about the greater increase of coal by oxidation? A. I think he did.

Q. You think he did? A. Yes.

Q. Did you tell him to look at that book and say, here, gentlemen oxidation instead of increasing the weight of coal decreases the weight—did you tell him that?

A. If I told him anything I would have told him what I told you yesterday when you referred me to the data.

Q. You would not tell them what you have told to-day, or what the book says?

A. I would be perfectly willing to, if the question came up. **[1495—1432]**

Q. If the question came up? A. Yes.

Q. Why is it that a small lot of coal is more subject to oxidation than a large quantity of coal?

(Testimony of S. W. Parr.)

A. If the small quantity of coal is placed under circumstances where oxidation is carried on, those facts would have to be observed and taken into account.

Q. Where does the oxygen come from that causes the oxidation or burning up of the coal, slow burning up of the coal? A. It comes from the air.

Q. Now, then, the air can more rapidly reach a 100 lb. lot of coal than it can a 1000 ton lot of coal, can it?

A. If the element of time is taken into account, there would be very little difference.

Q. That is, if a 100 lb. lot may be consumed to the extent of 10 per cent you say at given time, a thousand ton lot would take years and years to burn to the same extent, would it not?

A. The question related to the availability of oxygen for a small lot and a large lot. The transmission of oxygen to the mass of a large pile may take a longer time, but the supply of oxygen is inexhaustible.

Q. How did you handle this coal which you put upon the roof in Illinois? I refer to your table 3; in an open box, on the roof, 311.8 pounds, put on the roof; what size box was that?

Mr. OLNEY.—What page is that, Mr. Sullivan?

Mr. SULLIVAN.—Oh, you have it, have you?

Mr. OLNEY.—Yes, I have it.

Mr. SULLIVAN.—Page 18. **[1496—1433]**

A. I wish to refer to your question of a moment ago, as to the authorities and references.

Q. No, please answer the question; we will take

(Testimony of S. W. Parr.)

one subject at a time.

A. Bulletin No. 28 of the Experimental Laboratory of the United States Bureau of Mines, page 22. In the column here under experiments which were conducted with a view to all the varying conditions, the column is headed "Increase in weight due to oxidation, per cent": Illinois No. 9a, 1.68 per cent. Illinois No. 9b, 1.78 per cent. The same, 2.47 per cent. The same, 1.34 per cent. Illinois No. 11b, 1.03 per cent. Indiana No. 4, 1.32 per cent. Massachusetts peat, 1 per cent. North Dakota, lignite, 1.57 per cent. I skip 1, 2, 3, 4, 5, 6 samples, for the sake of time, in which the lowest is 0.4 of 1 per cent, but are all fractions of 1 per cent. Wyoming coal, with an increase in weight due to oxidation of 2.56 per cent. I repeat the statement I made before, that all the conditions must be taken into account, and these are carried out under those conditions.

Q. This is your language in this book here, is it not, Mr. Parr— Will you please let me look at that book, page 31 of your book: "Coal of the type found in Illinois and neighboring states is not affected seriously during storage, when only the change in weight and loss of heating power are considered. The changes in weight may be either gains or losses, and are probably never over two per cent in the period of one year." Is that correct?

A. That is correct when all of the circumstances are taken into account.

Q. So that you say when the coal is stored for the period of one year, the change in weight may be

(Testimony of S. W. Parr.)

either gains [**1497—1434**] or losses?

A. All Illinois coal has a content of water not less than ten per cent, but usually about fifteen per cent, as it comes out of the mine. All of the circumstances of the same must be taken into account.

Q. Is not high moisture coal more subject to oxidation than low moisture coal?

A. The lignites are more subject; they are high moisture coals, if you call them.

Q. I am talking about high moisture bituminous coals; aren't they more subject to oxidation by reason of the excess of water in them?

A. You will recall that oxidation takes place—

Q. Will you please answer the question?

A. I am answering the question, I hope. I am trying to answer it.

Q. You can answer that by saying "Yes" or "No," Professor, and then explain afterwards. Aren't bituminous coals more subject to oxidation by reason of the excess of water in them?

A. It does not necessarily have reference to the amount of moisture, but the amount of sulphur, which is the largest element in the case.

Q. The sulphur is one element in the case, but as I understand it, oxidation takes place by the union of oxygen with the sulphur in the coal, and also it takes place by reason of the union of oxygen with the substance of the coal, itself; is not that the fact?

A. Those two processes are going on.

Q. Is not the substance of high moisture coal more liable to oxidation in high moisture coal than the sub-

(Testimony of S. W. Parr.)

stance of low moisture coal?

A. I cannot say as to that directly; [1498—1435] the presence of moisture, or the absence of moisture, in itself, does not necessarily indicate the amount of oxidation of the carbonaceous matter. The presence of moisture or the absence of moisture does facilitate in a great degree the oxidation of the sulphur, and the resultant of those processes is termed the oxidation of the coal.

Q. Haven't you stated and written to the effect that high moisture bituminous coal is more subject to oxidation by reason of the excess of moisture in it than low moisture bituminous coal?

A. High moisture has in itself no oxidizing power.

Q. Does not the presence of an excess of moisture in coal aid the oxidation of the coal?

A. It does when it comes to the oxidation of the sulphur, but it has nothing to do with the facility of the oxidation of the carbonaceous matter.

Q. Then the oxidation of sulphur in coal is greater in high moisture coal than the oxidation of sulphur in low moisture coal; that is the fact, is it not?

A. Only in so far as the water helps out oxidation; if it is there in large amount, the oxidation may proceed farther, but there are other elements that must be taken into the case besides mere matter of water.

Q. But water must be taken into consideration as one of the elements?

A. There must be water to start the process.

Q. What is the sulphur content of the Illinois coal that was analyzed by you and experimented on by you

(Testimony of S. W. Parr.)

as shown by this Table C?

A. It ranges from a fraction of 1 per cent to 4, and 5, and sometimes 6; the latter is a rare instance, however. **[1499—1436]**

Q. And the sulphur content of the Nanaimo coal is very small, is it not?

A. It is smaller than the average of what I have given.

Q. Is not the sulphur content of Nanaimo coal smaller than the sulphur content of any of the Eastern coals that you have experimented on?

A. We have many Illinois coals which I think are included in that table, wherein the sulphur content is about the same as the Nanaimo coal.

Q. The sulphur content here of the Nanaimo coal varies 0.72 of 1 per cent up to 1.24 per cent; is not that a low average of sulphur content in any coal?

A. It is reasonably low, compared with the coals of the East.

Q. And that kind of coal containing such a small quantity of sulphur, is not as liable to oxidation as coal containing sulphur ranging, as you say, from a fraction of 1 per cent up to 5 and 6 per cent; that is a fact, is it not? A. That does not follow at all.

Q. It doesn't follow at all?

A. If all of the sulphur oxidized in the Nanaimo coal, and only a fraction of the sulphur of a higher sulphur content, the two cases would be the same.

A. Four per cent sulphur coal might only have 1–10 of the sulphur oxidized.

Q. That would depend on the amount of oxygen

(Testimony of S. W. Parr.)

that gets to it, would it not?

A. It depends on the amount of opportunity it has, temperature, moisture, and time, and fineness of division.

Q. Submit a high percentage sulphur coal to the same oportunities that a low percentage sulphur coal is subjected to, and the high sulphur coal will oxidize proportionately greater, **[1500—1437]** will it not, to a greater extent, rather?

A. That would not necessarily follow, at all. If the two coals were ground, for example, to the same degree of fineness, and subject to the same temperature and the same action of the elements, they would proceed reasonably together, but one would get through quicker than the other.

Q. What do you mean?

A. One would exceed the oxidizing capacity.

Q. Which one would?

A. The one that has the least check, the smallest amount of oxidizing material; but if you are going to oxidize all, for instance, if a 1 per cent oxidizing coal—and as I have already testified, the increase in weight would be five or six times the weight of sulphur oxidized, you can see at once that you would be going to an increase of weight which would be five or six per cent of the coal. Those cases would be very rare. I will say, however, that in a number of Canadian experiments, the increase in weight is shown to be as high as 11 per cent. I regret exceedingly I do not have those references here, but I can get them.

(Testimony of S. W. Parr.)

Q. That is due to and is on account of the sulphur content?

A. That is due to both the oxidizing substances of the coal.

Q. I will put this question to you: Won't coal that contains as high as 5 per cent of sulphur increase to a greater extent in weight by oxidation than coal that has only 1 per cent of sulphur?

A. That does not follow at all.

Q. That does not follow at all?

A. It does not necessarily follow.

Q. Supposing that all the sulphur in one coal oxidized **[1501—1438]** and that all the sulphur in another coal oxidized.

A. Then the one that had the highest amount of sulphur would have the greatest increase in weight.

Q. You read from the table, here, showing the percentage of increase in weight according to a report that you held in your hand a little while ago, showing that the increase in weight was as high as 4 per cent in some cases—I mean 2 per cent, rather, the increase in weight was from a fraction of 1 per cent to 2 and a fraction per cent; do you know how long these coals were subjected to exposure?

A. I do not have it in mind, I do not recall.

Q. Do you know whether they were subjected to exposure for a year or more?

A. I do not have the data in mind.

Q. Don't you know that in order to intelligently explain to the jury the degree of oxidation you should explain to the jury the length of time the coal is sub-

(Testimony of S. W. Parr.)

jested to exposure? A. I think that would be fair.

Q. You didn't do it in this case, though, did you?

A. I did not.

Q. Have you any means at hand now of learning, so as to explain to the jury how long these coals were subjected to exposure?

A. I think the bulletin probably states it.

Q. I wish you would point it out. But, before you point it out, I wish to direct your attention to the fact that the sulphur contents of these coals was largely in excess of the sulphur content of the Nanaimo coal, ranging from 0.58 of 1 per cent to 6.86 per cent. From that bulletin, there, show me how long that coal was subjected to exposure.

A. I read as follows: "The oxidation changes in every case **[1502—1439]** are sufficiently large to be of practical importance. The smallest change, that in the Ohio No. 5 sample, being 0.53 per cent, while the Illinois No. 9b sample showed a change of 2.47%. The Wyoming No. 3 sample showed an increase of 2.56 per cent in weight, and a decrease of 205 calories in heating value. The changes in weight correspond to a decrease in heating value of 144 calories, the final calorific value obtained being 75 calories lower than is accounted for by the changes in weight. Further results along these lines are desirable, but the values already obtained show very clearly that old samples of coal cannot be regarded as representative of the original coal in composition or in calorific value."

Q. Professor, do you think that is an answer to the

(Testimony of S. W. Parr.)

question I have put to you? A. I think it is.

Q. Then I will have to repeat it. It is not. Will you point out in this book any reference to the time that this coal was subjected to exposure?

A. I think I can find it, if I look through the details of the analyses.

Q. I will withdraw that question for the present. Do you know, and will you state, whether that coal was subjected to certain extraordinary heat, for the purpose of bringing about oxidation?

A. I will not state it.

Q. Or was that oxidation caused by the exposure to the air, and subjection simply to atmospheric influences?

A. I do not recall the details of the experiment.

Mr. OLNEY.—Now, Mr. Sullivan, right at this point, if you will kindly allow the witness to refer to the Bulletin and find there the conditions under which these samples were taken, I think that would be fair. **[1503—1440]**

Mr. SULLIVAN.—I wish he would.

Mr. OLNEY.—Q. Professor Parr, if you will begin at page 19, at the bottom of the page, you will find there a statement of the conditions, and I will call your attention to this, that in the table of percentages, on page 22, you will find the laboratory number, which gives the number of the sample, and then you will find the same number in the tables given on page 20, and from that you will be able to answer Mr. Sullivan's question as to the length of time that these samples were undergoing these tests.

(Testimony of S. W. Parr.)

A. I take, for example, the first one, Illinois coal, No. 9a, increase in weight due to oxidation, 1.68 per cent; Illinois No. 9b is given as having 0.17 per cent change, increase in weight—that would be change in weight.

Mr. SULLIVAN.—Q. 0.17 of 1 per cent?

A. Yes, sir, 0.17 of one per cent, for a period of four days, 0.47 of 1 per cent increase for a period of 20 days; 0.46 of 1 per cent after a period of 42 days; 0.19 of 1 per cent in 25 days; nothing after 172 days.

* * * * * * * * *

Q. Where was it you read from, Professor? Can you tell from this book the quantity of coal used in the test? A. I can.

Q. I wish you would find it.

A. "In order to investigate the question of the extent of alteration of the samples when kept in a finely powdered condition as prepared for analysis, portions of a number of such samples were put in weighed bottles, which were securely closed with rubber stoppers. These bottles were kept in the laboratory and weighed from time to time," and so on. [1504—1441] The amount of the material is not given in that part of it.

Q. The coal was pulverized, was it?

A. It was. That item with regard to the quantity of coal is undoubtedly there somewhere, if the time of the Court can be given to look it up.

Q. Did oxidation, in that case, then, take place by virtue of the moisture in the coal, itself, and not by reason of the moisture in the atmosphere?

(Testimony of S. W. Parr.)

A. All coals have sufficient moisture to start this oxidation and keep it up.

Q. Now, let us see the change in one of the samples; you do not know the quantity in the sample, do you? A. It is undoubtedly given there.

Q. In four days, there was a change in weight of 0.17 of 1 per cent; that is correct, is it? A. It is.

Q. In 20 days, there was a change of 0.47 of 1 per cent; in 42 days, there was a change of 0.46 of 1 per cent.

A. There is another case where a change in 7 days was 0.22 of 1 per cent.

Q. In 25 days, there was a change of 0.19 of 1 per cent. In 172 days, less than 6 months, there was no change at all. A. No additional.

Q. In 124 days—about four months, say—the coal, instead of increasing in weight, diminished in weight, did it not, according to the sign there?

A. There was no further increase.

Q. There was a decrease of 0.33 of 1 per cent, was there not, according to the table—minus .33, does not that indicate that at the end of 124 days, instead of being an increase in weight there was a decrease of about 1/3 of **[1505—1442]** 1 per cent, according to this table?

A. That might be; I would want to look into the details to see what that means.

Q. That is what it means. You read the table a moment ago.

A. On the face of the table, and without further explanation, it would seem to indicate there was a de-

(Testimony of S. W. Parr.)
crease in the weight.

Q. A decrease in the weight, in 124 days?

A. No, in the last period.

Q. That is 124, isn't it? That may be a mistake, may it? I don't know. It says there, 124 days between weighing. No, it cannot be a mistake, because you see here 42 is ahead of 25.

A. But 172 is more than 25.

Q. But here is the case of 42 days exposure and there was a change of 0.46 of 1 per cent; now, here, there were 25 days, and there was a change of 0.19 of 1 per cent. So that must be 124 days, as shown upon the table here? A. Yes, sir.

Q. And in 124 days, instead of there being an increase in weight resulting from oxidation, there was a decrease of 0.33 of 1 per cent; that is a fact, is it not?

A. That seems to be indicated in the table. It is the only case in all the list, where there is such a decrease, and it should be interpreted, in view of the possibility of some external condition.

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Mr. SULLIVAN.—Q. I will ask you to look at this table and state if the moisture content of the coals that were analyzed **[1506—1443]** was not largely in excess of the moisture content of the Nanaimo coal?

A. I think I must be permitted to look into the table, and not answer offhand.

Q. Certainly, just look at it. Explain to the jury the moisture content of those exhibits.

A. The moisture content in the case of the Illinois

(Testimony of S. W. Parr.)

coal is 5 per cent; the next coal has a 12 per cent moisture content; the next one 11 per cent moisture content, 4 per cent, 4 per cent, 3 per cent, 3 per cent; West Virginia 1 per cent.

Q. Where the moisture content is very low as shown here, the oxidation was very low, was it not? For instance, I will take here, take the case of the West Virginia coal, West Virginia No. 13, the moisture content was 1.03 per cent; the samples are not put down here—yes, they are, too, 24 days; the moisture content of coal when examined after 24 days was 1.16 per cent? A. It is.

Q. The increase in weight was 0.24 of 1 per cent; after 191 days, the oxidation was 0.15 of 1 per cent, was it not? A. Additional.

Q. And the moisture content was 1.21 per cent, was it not? A. It was.

Q. And after 108 days, the increase in weight was 0.07 of 1 per cent; that is a fact, is it not?

A. The further increase.

Q. And the moisture content in that case was 1.09 of 1 per cent; that is the fact, is it not A. It is.

Mr. OLNEY.—Mr. Sullivan, it is only fair in that connection that you should read the total increase in weight during that period. You have simply read the increases during certain periods, which are to be added together. [1507—1444]

Mr. SULLIVAN.—The total increase of weight, that is, for the different weights taken at different times—not an average—is 1.37 per cent—no, just wait a while, West Virginia coal 0.46 of 1 per cent.

(Testimony of S. W. Parr.)

A. And the water content of that coal is 1.3 average, is it?

Q. It is a little more than that, I think, Professor; it is about 1.12. Now, where are the other figures which we gave a little while ago? What sample is that?

A. This is the Illinois coal, and this is the same here.

Q. 9a. Now, take the case of your experiment in Illinois, Professor, will you let us look at that book during the recess, please? A. Certainly.

Q. I would like to look at it now. Now, you say there were certain conditions existing when you made the tests for oxidation as shown in table 3 of your own book. What were those conditions, will you explain, Professor?

A. As I recall, the coal was put on the roof, subject to the winds and rain, in boxes, and carried along under those circumstances, which were entirely different from some other conditions which might have been put in operation.

Q. The first experiment of the screenings from Williamson County in an open box, on the roof, the table shows that there were 311.8 pounds in the box. There was one box, was there? A. One box.

Q. How large a box was it?

A. Oh, it was about 18 inches by two feet, probably.

Q. And how deep was the coal in it?

A. I think six or eight inches.

Q. And you say 18 inches by 2 feet—how many pounds in an ordinary sack of coal? About 100

(Testimony of S. W. Parr.)

pounds, **[1508—1445]** aren't there?

A. It depends on the size of the sack.

Q. Coal is generally sold in 100 pound sacks, is it not?

A. Coal is never sold in our country in sacks.

Q. It is in this country. It is very valuable out here.

A. I don't know what the sack there would refer to.

Q. This shows 311.8 pounds. As well as you can recollect, give us the dimensions of that box; you say it was 6 inches deep; it must have been more than 18 inches by 2 feet.

A. It probably would be greater than that; it would have to be a box that would contain about six cubic feet of space, a box 2 by 3 feet, and about a foot deep.

Q. That is considerable exposure to the coal, is it not? A. Yes.

Q. That was subjected to the rain, the snow and the wind? A. Yes.

Q. And the opportunities for oxidation were manifold, were they not? A. Presumably.

Q. If you would take a box of coal of the same quantity, the Nanaimo coal, the same quantity, and expose it on the roof, the opportunities would be the same, would they not? A. Presumably.

Q. What else did you do with the coal at that time, until you reweighed it; that is, you weighed it once on December 15, 1908, and then reweighed it on the 17th of June, 1909, to ascertain the difference; was any-

(Testimony of S. W. Parr.)

thing done with it in the meantime? **[1509—1446]**

A. I think not. It might have been sampled, or examined in some way, but I do not recall.

Q. You have no recollection of having touched it at all, have you? A. I think not.

Q. And your impression is that the coal remained in that condition on the rood of the building for that period of time, some six months, and exposed to the elements? A. Yes.

Q. And if you conducted the same sort of experiment with the Nanaimo coal, the coal being of low moisture content, don't you think there would be a loss of weight, as there was in this case of the Illinois coal; in the Illinois coal there was a loss of 1.54 per cent; don't you think with the Nanaimo coal under the same conditions, and being a low moisture coal, there would have been also a loss of weight, and a loss of weight in excess of the loss of weight in this particular case?

A. It would be more reasonable to compare it with the same coal from the same country, carried on in an experiment in this other bulletin, which shows the records in tests under exact measurement as to oxidation. The book which you hold in your hand in another place in the table shows a decrease in calorific value as you pointed it out to me yesterday of one and a fraction per cent, and that must have been counterbalanced by some increase in weight in the sample to represent that decrease in calorific value. The relation between that experiment and the roof experiment, exposed to all of the

(Testimony of S. W. Parr.)

conditions which you have described, and which are put in the book, would bear no relation, and no proper conclusion could be drawn from them. **[1510—1447]**

Q. Were not the opportunities for oxidation in this given case appearing on page 3 greater than the opportunities for oxidation of the coal in the bunkers of the Western Fuel Company?

A. I cannot say as to that.

Q. Don't you think that the opportunities for oxidation in this particular case, where there was a loss of 1.54 per cent in weight were greater than the opportunities for oxidation in the yard of the Western Fuel Company, where the coal was piled up and—

A. (Intg.) No offhand answer could be given to that question, because, as I have stated, the conditions involving oxidation are so variable that I am not willing to commit myself to a statement which does not take into account those conditions; it is not fair that I should do so.

Q. You have already said that the screenings oxidize more rapidly than the average coal; that is the fact, is it not?

A. The finer the coal, the more accessibility of oxygen to it.

Q. Now, don't you think in view of the fact that in this particular case the coal was screenings, that the opportunity for oxidation during those six months was greater than the opportunity for oxidation of coal, the average coal, in the yard of the Western Fuel Company during the period of six months,

(Testimony of S. W. Parr.)

where the coal was piled quite high?

A. Also the opportunity for the carrying away of coal by wind currents and other means would be correspondingly greater.

Q. The carrying away of coal by wind currents is not what you term oxidation, is it?

A. Well, say by rainfall and carrying away by a process of leaching and draining out. **[1511—1448]**

Q. What do you mean by referring to wind currents? Now, we are getting wind currents in this case. What do you mean by wind currents having any effect on oxidation?

A. I didn't say they had any effect on oxidation; I said the possibility of the carrying away of the coal.

Q. Oh, the carrying away of lumps of coal.

A. Certainly. Coal flies in the air under proper conditions very readily.

Q. Does coal fly in the air in San Francisco under any conditions?

A. Down on top of the bunkers, it does.

Q. I suppose in Illinois, it doesn't take wings at all, does it? It is not affected by the wind there.

A. If it is powdered finely enough, small particles are carried away by the wind in Illinois just the same as they would be in San Francisco.

Q. Why was it, in reading from this book a little while ago, Professor, you stopped at the 172 days experiment which showed there was no change in the weight, and did not read on to show there was a decrease in weight upon the 124 day experiment, the

(Testimony of S. W. Parr.)

last figure in the column, showing there was minus 0.33?

A. It may have been because you wanted the book; I do not recall.

Q. You read that while I was sitting down here, didn't you? You read all those figures, excepting that last figure, showing minus 0.33 while I was sitting here, didn't you?

A. Will you bring me the book again—may I have the book again?

Q. Yes.

A. Very well, do you wish me to read that additional item?

Q. It is not necessary to read it now. **[1512—1449]**

Mr. McCUTCHEN.—I think the witness ought to have the right to read it, Mr. Sullivan; you have cast an imputation against him.

Mr. SULLIVAN.—I have asked him why he didn't read it. I have read it already, and he has read it already. You can read it now if you want to.

A. One hundred and twenty-four days, minus 0.33 per cent. If this is an exceptional case, it may have occurred to me at the time—I do not recall that I had any thought in the matter—that it would not be fair to read it. I see, however, that there is one other case, that of Illinois coal #1639, in which there is a total increase in weight due to oxidation of 1.54 per cent, and in the last 127 days, there was a loss, a decrease in weight of 0.12 per cent. I hope that by reading these two unusual cases, I will remove any reflection

(Testimony of S. W. Parr.)

on my wish to do the proper thing in the matter.

Q. And these were all coals containing greater moisture than the Nanaimo coal, from 4.03 up to 4.53. A. The last two coals that I have read.

Q. Yes. This is what you read, here. A. Yes.

Q. The moisture content of the Nanaimo coal is what, according to your recollection?

A. From 3 to 3¼ or 3½, some of them were a trifle under 3 per cent.

Q. One sample, I see is 3.55, one is 2.97, one is 2.54, car in shed; 4.44 open car.

Mr. OLNEY.—Q. 2.54 was a car of dry coal, was it not, which you refer to?

A. Which had been under cover.

Mr. SULLIVAN.—Q. These samples here, taken where the moisture content is shown to be as high as it is, were all dry coal, were they not, or were they the coal taken from the mines? **[1513—1450]**

A. They were coals with the content of moisture indicated in the table; I cannot say whether they had been artificially dried, or whether they were naturally, without looking at the book.

Q. Look at the book, now, and see if these coals were not artificially dried.

A. Illinois coal No. 1635—well, I think the reply is entirely immaterial, for the reason that there is no coal in Illinois with a moisture content so low as 5 per cent without artificial drying; but Illinois coal No. 9b has given here a moisture content of 12.34 per cent, an average of 12½ per cent, possibly; that coal probably was not artificially dried, but was un-

(Testimony of S. W. Parr.)

der normal conditions.

Q. Which was that, Professor?

A. Illinois coal No. 9b.

Q. What was the moisture content there?

A. 12¼ on the average.

Q. But where the percentage comes down to four or five per cent, it was artificially dried?

A. Yes, sir.

Q. And dried in a room where the heat was 105 degrees centigrade?

A. That would be the boiling point.

Q. Yes, I know, but what was the temperature of the room in which the coal was dried?

A. The drying of those coals would be in a room at about 85 degrees Fahrenheit.

Q. Don't you make some tests for the purpose of drying where you seek to dry up all the water out of the coal, and dry it at 105 degrees or 100 degrees centigrade?

A. That is for the drying out of all of the water, but you will notice that the water here was not more than half, at the most, dry. **[1514—1451]**

Q. Now, one more question on this particular subject and I will drop this subject and go to another: As I understand you, you say where the calorific value, or heat value of coal is reduced, that shows there has been oxidation; is that a fact?

A. There has been oxidation, or some condition corresponding to that.

Q. Where the heat value of coal is reduced by reason of oxidation, does the coal always gain in

(Testimony of S. W. Parr.)

weight by reason of the oxidation?

A. That would be subject to different conditions.

Q. Well, take the ordinary conditions of coal exposed to the elements, as it was in the case of your experiments, would the reduction in the heat value indicate an increase in weight resulting from oxidation? I so understood you to testify yesterday; I may have been mistaken, though. What do you say now, Professor?

A. Well, as a general proposition, those heat values that are given there are reductions in heat value due to some increase in weight of material that is not combustible.

Q. Well, your opinion is, professor, that where the coal is reduced heat value a certain percentage, that ordinarily there is an increase in the weight of coal resulting from oxidation; is that a fact?

A. That would be a general proposition; there may be certain circumstances under which that would not be true.

Q. Take the case of W. A. Powers, the chief chemist of the Santa Fe Railway Company, who made these tests, exposing 100-pound lots of coal to the elements for seven months. In those experiments it appears that **[1515—1452]** the coal stored in the open air lost in weight from 0.60 of one per cent to 4.78 per cent, and 1.10 per cent to 9.40 per cent of calorific value; now, there is a case where the coal was selected from five states, and in those instances here, the coal not only lost in calorific value, or heat value, but also lost in weight, ranging

(Testimony of S. W. Parr.)

from 0.60 of 1 per cent, to 4.78 per cent. Was that an extraordinary occurrence? Here we find, after this experiment was taken, and which included coal from Illinois—your own state, Missouri, Kansas, Colorado and New Mexico, that not only was there a decrease in the calorific value of the coal, but there was also a decrease, a very marked decrease, in the weight of the coal, a decrease ranging from 0.60 of 1 per cent to 4.78 per cent.

A. I should have to know all of the circumstances that entered into the determination of the heat value of the coal.

Q. You did know at the time you wrote this book, where you refer to Powers' experiment, did you not? You knew the conditions, then, did you not? A. What book are you referring to now?

Q. I am referring to Bulletin No. 38, on the Weathering of Coal, series of 1909, by S. W. Parr and W. F. Wheeler. That is yourself, is it not, S. W. Parr? A. That is myself; yes, sir.

Q. Now, at the time you wrote this book, and referred to this experiment showing a decrease in weight after seven months, did you not know the conditions under which those experiments were made?

A. You say this is work done by W. A. Powers, of which I do not know anything about, and I do not think I **[1516—1453]** should be asked to testify in regard to it.

Q. But you say he was a pupil of yours, and you referred to his experiments, you must have got the data from him, or from some source, for the purpose

(Testimony of S. W. Parr.)

of inserting the history of the experiments in your books.

A. Well, it is possible that by looking into the circumstances under which that test was made, I might afford an explanation for it.

Q. Does not the explanation appear on the very face of the book, itself, showing that these 100-pound lots were exposed in the open air?

A. I should want to know more about the circumstances. As I have said before, there are many circumstances which might enter in, which would account for the difference.

Q. But you cannot refer now to any peculiar circumstances that would account for the difference?

A. I am not familiar with them.

Q. You were at the time you wrote the book, were you not? A. I presumably was.

Q. Do you know if you could get the data anywhere? A. I do not know; I possibly could.

Q. Don't you write these books, inserting in them sufficient explanatory matter to serve the purpose for which the books are written?

A. That is the effort.

Q. And here, you assume that the conditions—not having explained to the contrary, you assumed that the conditions were normal under which these coals were tested and exposed to the elements?

A. The conditions under which the book was written were to illustrate the decrease in heat value due to coal in storage. There is a popular impression that coals in **[1517—1454]** storage will re-

(Testimony of S. W. Parr.)

duce in heat value by as much as 50 per cent, sometimes the statements are as high as that. The book was primarily and directly a book to determine how great are the heat losses in coal. If you will examine the charts in the back of it, you will see that it is entirely devoted, that it is primarily devoted *that it is primarily devoted* to an illustration of what are the extremes in the matter of deterioration and loss of heat value of coal in storage under commercial conditions. There is no prime effort in the book to get at the particular matter of weight losses, that is incidental, and should not be taken in connection with the prime purpose of the bulletin. I think the highest decrease in heat value, as shown by the book, is perhaps about 3½ per cent. Now, the effort to correlate with that any exact figure as to changes in weight, any experiments which were carried out on the roofs of houses, and so on, and the circumstances that might enter into the case I do not think is a fair illustration. If the counsel desires experiments that have been carried out with this particular and specific point of view, there are plenty of them just in the same line as those already given by the United States Bureau of Mines.

Q. That is your entire explanation, is it, Professor?

A. It is not. It is a general statement of the conditions.

Q. Do you desire to make any other statement now, concerning the purpose for which this book was written? A. I do not.

(Testimony of S. W. Parr.)

Q. You say this book was written for the purpose of explaining to the world, the dealers in coal, the buyers, **[1518—1455]** and sellers, and dealers in coal, the calorific effect upon coal resulting from weathering; is that so? A. Yes, sir.

Q. And not for the purpose of showing any change in weight resulting from weathering; is that a fact?

A. That is the primary purpose of the book.

Q. I refer you to page 11 of this book, and read as follows:

> "IV. Storage Conditions Object of the Experiments.—The object of these experiments was to determine the change in weight, the change in calorific value and the amount of disintegration that are liable to occur in the grades of coal found in Illinois and neighboring States under different conditions of storage, (1) in the open air in piles; (2) in covered bins; and (3) under water."

You state the first purpose to be to determine the change of weight resulting from storage; are you not mistaken in the explanation you gave a few minutes ago?

A. If you recall where you said that these relate to piles in storage in the open air, where it is subject to all kinds of modifications, which would result in the change in weight.

Q. That is all the explanation you want to make now concerning the purpose for which this book was written; is that so?

(Testimony of S. W. Parr.)

A. I think so, I think that is sufficient.

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Q. Now, Professor, I understand you made some experiments with the tubs down on the barges of the Western Fuel Company. A. I did.

Q. Did you make any experiment with tubs for the [1519—1456] purpose of ascertaining the difference in weight between a tub that was filled and a tub that was partially filled?

A. It was for the purpose of determining the gravity tipping point of the tub; and for that purpose some of these experiments were made.

The tests were made on January 30th and 31st, 1914, on the barge "Comanche." Tub No. 1 was filled to the top with the regular average coal of the barge. When so filled it weighed net 1390 pounds. I also weighed that tub at the point to which it would tip by gravity, being 1,270 pounds, or 120 pounds less than the net coal when the tub was full. I did not make specific measurements to determine how far from the rim of the tub the top of the coal was when the tub was thus partially filled, but I presume it would be an inch. The tare was 730 pounds, but the weight I have given is net. I made another test with that tub to determine the difference in weight between the tub when full and when partially filled. The same tub Number 1 was filled full, net 1,340 pounds. I removed the coal just to the point where it would tip by gravity and its weight was 1,270 pounds. The difference between the full load and the gravity tipping point was thus 70 pounds.

(Testimony of S. W. Parr.)

Q. Now, did you make any experiment where you filled the tub at the mouth, filled the tub to the rim at the mouth and the coal at the back was a foot or so, the top of the coal at the back was a foot or so from the top; that is, did you fill the tub with coal in a slanting position, the top of the slant being at the mouth of the tub and the bottom of the slant being at the back of the tub, and see whether or not the tub so filled with coal would not tip?

A. I made an experiment of that sort with the coal one foot below the edge, but, of course, not **[1520—1457]** for a gravity test, but for a tipping test on the rig.

Q. Was it filled to the mouth, was it up to the top of the tub at the mouth?

A. I don't think that the slant of the coal would admit of it being filled to the lip and one foot below at the rear.

Q. You don't think so? A. It might be.

Q. Don't you know it could be done?

Mr. McCUTCHEN.—The witness has just said he did not think it could be done.

Mr. SULLIVAN.—Well, I will ask him again, don't you think it could be done; that is, the coal at the mouth would be even with the rim of the tub and at the back would be about a foot from the top of the tub, just as it is scooped up from a bank of coal in the hold?

A. I think the angle of repose would not be caused in that case.

(Testimony of S. W. Parr.)

Q. Will you explain what you mean by angle of repose?

A. That angle at which the coal will remain in position without rolling down.

(Witness continuing.) There were only two tests as to the point at which the tub would tip by gravity alone, and I have given you those two tests. By gravity alone I mean that when the tub is suspended by the hook it is filled to a point where the tub, without striking the bunker, would turn over and dump itself.

Mr. SULLIVAN.—Q. Will you give me the data upon the other barges, Professor?

A. The barge "Nanaimo," January 31, 1914; the tub was filled full, having a net weight of coal of 966 lbs.; in attempting to get a point at which the tub would tip by gravity alone, it had no such points, no matter how much it was overloaded. Of course, putting the load on evenly over the surface; it was heaped up to 4 inches or 5 inches above the top of the curve until the net weight was 1,036 lbs. At this point the tub reached just a **[1521—1458]** position where it would overbalance.

Q. That is without striking any obstacle at all?

A. Without striking any obstacle, the gravity test.

Q. What was the tare at that time, the tare of the tub? A. Five hundred and seventy-four lbs.

Q. What other test did you make? What tub was that? Tub No. 1?

A. I think it is. I would like to verify the num-

(Testimony of S. W. Parr.)

ber of the tubs, as I have only recorded the net weights. Tub No. 4. We only worked with one tub on the "Nanaimo."

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Q. Can you give me the tubs that you tested on the "Theobold"? We won't go into these dumping tests, but give me the number of the tubs you tested?

A. The "Theobold," tub No. 2.

Q. What was the tare of that tub?

A. Five hundred and ninety lbs.

Redirect Examination by Mr. OLNEY.

There are two sizes of tubs on the various barges but each barge has the same sized tub. The smaller sized tub will be found on the "Nanaimo" and one other barge. When we filled these smaller tubs up to 1½ inches from the rim the tub would not dump when thrown up by the rig the first time, but did trip on the second effort. Then we filled the tub to one foot below the edge and it was impossible to get it to trip at all. When that tub was filled to the top it weighed 830 pounds net, but filled to within a foot of the top it weighed 554 pounds net coal. The tub which we were able to trip on the second trial, being that which was filled **[1522—1459]** to within 1½ inches of the rim, was 100 pounds short of the full tub. We did not lower the tub to see how much the coal had been displaced by the first bump, but on the second bump it tripped and the bump was the usual one carried on by the regular men in the employ of the company.

As to the larger tubs, we took Number 2 on the

(Testimony of S. W. Parr.)

"Comanche" as a representative. A full bucket of 1,400 pounds net would not dump at 1,150 pounds, but would dump at 1,200 pounds. This shows a difference between the full tub, 1,400 pounds, and the dumping point, 1,200 pounds, of 200 pounds. At this point the tub measured 4 to 5 inches below the edge on all sides, counting the curve and front and rear.

When I testified on cross-examination that I had been employed as an expert in this case at $25 a day and expenses and upon other conditions, I meant by the other conditions that I was to determine facts and testify as to them without regard to any bearing they might have either for or against the parties interested. That was the only condition under which I would accept employment by the Western Fuel Company. I was told that the parties interested in the case wished to know the exact facts in the matter without regard to their bearing on any questions that might be involved.

I took samples from the three cars at Nanaimo, that is, a separate and distinct sample from each car. Each sample, therefore, contained coal from one car only, and not from all three. I examined the rain records at Nanaimo to ascertain what the rainfall had been for a short time previous to my coming there and I found that the rainfall on August 16, 17 and 18, 1913, was .51 of an inch. On July 11, 1913, the rainfall was .59 of an inch. So far as the weather report indicated, the intermediate days were **[1523—1460]** dry.

(Testimony of S. W. Parr.)

Q. Is it possible to formulate a rule, a general rule, as to the ratio of increase in weight due to moisture taken on during storage, for instance, as between coals with a natural high moisture content and coals with a natural low moisture content? I do not mean the percentage of difference in the ratio, but simply is there any relation or ratio between the two?

A. There is a general statement, a general law which could be stated.

Q. Will you state it, please?

A. The coals of low moisture content are low in moisture and are at the lower limit, so that anything that could happen to a low moisture content coal under conditions exposed to the weather, would be an increase; now, a high moisture coal is at the upper limit and any additional water that comes on has a greater opportunity for getting away, so that those coals tend to drop down in moisture; the tendency to evaporate high saturated coal is relatively greater than a coal that is already down to the limit. So that a ratio, such as you refer to, it does not seem to me could be established; for example, there are many coals that come out from the mine with 12 to 15% of water; now, 5% additional water would we will say, be from 17 to 20 per cent of water; a coal carrying that amount of water would certainly be much more easily reduced in weight by exposure to the air because it is far above the ordinary saturation point than coal which had originally 3 per cent and had the same amount of water added.

(Testimony of S. W. Parr.)

Q. That is, 5 per cent?

A. 5%, which would be **[1524—1461]** in that case 8%; a coal that had 8% of water added to it, would not lose moisture so readily as a coal that was high and saturated; in other words, a low moisture coal that starts out under conditions which are at the minimum could not lose any more moisture, it is already dry and is at a near balance with the atmosphere and weather conditions. The same thing could not be true of a coal that starts out with high moisture and has water added to it.

Q. You stated, in answer to a question asked by Mr. Sullivan, that there was no rule as to whether a loss or gain would take place in the weight of coal upon exposure to weather; that is, the coals were so variable in that respect that no rule could be enunciated; confining the subject, however, to coals with the inherent low moisture content is the same true, is it or is it not in that case possible to say whether or not the coal on exposure to air would lose in weight or increase in weight, and which way it would go?

A. Well, I think that question was partly covered in the previous answer, to the effect that starting out with a coal which is at the minimum all the changes in such a coal would be gains, so far as moisture would be concerned.

Q. Would you expect to find that gain upon the exposure of such coal in storage to normal weather conditions?

A. The same answer would apply to that question,

(Testimony of S. W. Parr.)

namely, that since it starts out with the minimum amount of moisture, the gain while similar would still be a gain.

Mr. OLNEY.—Q. You spoke, in answer to a question by Mr. Sullivan, of a tolerance being given on account of moisture, certain coals in the East having a tolerance allowed on account of moisture; what do you mean by that? You said a **[1525—1462]** tolerance of from 1 to 15%?

A. I mean by that that under certain conditions, such as coal delivered from a washery, the finer sizes may carry so much water, and being of such low commercial value it has come to be an established practice to allow these tolerances; for example, coal No. 5, in the Southern Illinois field and in some other places is what we may call a buckwheat size and has all the fine material in it; now, while it is a very pure quality of coal, it is, as I have said, of low commercial value, and while it has inherently 14 or 15 per cent of water it comes from the washery with an addition to that of what is recognized as approximately 15 per cent of water; that is to say, from 28 to 30 per cent, of this material is water. Now, a tolerance is allowed in the weight of such coal of 15 per cent. A mixture of the next size, largely—

Q. (Intg.) Permit me to interrupt you: Allowed by whom, and what?

A. It is allowed by the railway weights to the shippers.

Q. That is, from 1 to 15 per cent is deducted from

(Testimony of S. W. Parr.)

the weight of the coal to allow for the water that is in it?

A. Not exactly that; No. 1 coal has a little tolerance, of one per cent; No. 2, 2 per cent; No. 3, 3 per cent; No. 4, 4 per cent; No. 5, 15 per cent.

Q. Take the case of the 15 per cent, the tolerance which is allowed, is a deduction on the weight of the No. 5 coal of 15%

A. In order to get the weight of the coal without the water.

Q. Without the water, as a commercial proposition? A. That is the case. **[1526—1463]**

Q. And the freight rate is based on that weight?

A. It is.

Q. Now, in answer to a question of Mr. Sullivan you stated that the matter of computing the probable percentage to which the coal would increase in weight, or by which the coal would increase in weight, even with the coal moving from time to time, that is, the coal being purchased and put in storage and other coal being taken out and sold, was a matter of comparatively simple computation. Will you just explain how that is?

A. If we have a given area of coal upon which water is being discharged, we will say, for simplicity of the illustration, from a pipe delivering a given amount in a given time, the discharge of that pipe over an hour will be a given amount. Suppose it increases the weight of that mass of coal, for convenience in figuring, of 10%; now, supposing instead of an hour's time two such areas of coal are placed

(Testimony of S. W. Parr.)

under the water, one for half an hour and the other for half an hour, we have an increase in weight of the two masses of coal of 5%; and so on the illustration would apply. Now, suppose there is a continuous movement of coal we will say on a belt, with a given area and a given fixed discharge of water, if the belt moves forward and the coal proceeds with a fairly uniform rate, the amount of water which will be discharged upon the coal will be a comparatively fixed ratio as between the mass of coal that has passed in a given time and the water that has flowed from the supply. That it seems to me would illustrate **[1527—1464]** the essential factors that are involved in making such a calculation.

Q. Now, take and apply those factors to this particular case, where, as the question was put to you, there were some 2,000,000 tons of coal imported and sold during the 6 or 7 years involved—and the average amount of coal carried on hand was about 25,000 tons not under cover and 7,000 tons that were under cover; now, will you explain how with those figures and the coal constantly moving—that is, 2,000,000 tons having moved in 6 or 7 years, you are enabled to get a reasonable percentage or get at a percentage of increase within reasonable limits?

A. Well, we have here the factor of area again exposed to a constant fall of water month by month; at least we start out with factors of that sort; now, we say that there are 25,000 tons of coal in process of movement because at the beginning or at the end of a given period of time or at any period during

(Testimony of S. W. Parr.)

the year that amount of coal has passed by; if we take as a unit of area to start out with of a bunker for example which measures very approximately 20 by 40 feet—as a unit for illustration; now, the area of a bunker of that sort is 800 square feet; the rainfall upon such a bunker for the year's period is very nearly a ton to the inch, or for convenience of illustration—2 tons to the inch, or 50 inches of water.

Mr. SULLIVAN.—Q. Two tons to the inch?

A. Eight hundred square feet of surface in the course of the year will deliver that amount of water; now, we have **[1528—1465]** the factors of the amount of coal passing in review, and the water which falls upon it, which enter into the calculation.

Mr. OLNEY.—Q. Just go on with that illustration, Professor Parr; suppose you go ahead with the illustration of the bunker, for instance. I would like to interrupt you for a moment though: what do you mean by 2 tons of water to the inch? Upon what basis is that?

* * * * * * * * *

The COURT.—Eight hundred square feet covered with an inch of water.

* * * * * * * * *

A. If you wish the calculation on which that is arrived at, an area of 20 by 40 feet gives 800 square feet; now, 800 square feet, one foot in depth of water—

The COURT.—Q. You mean one inch?

A. No, I am figuring now for convenience to get it into cubic feet; 800 square feet, if it were a foot

(Testimony of S. W. Parr.)

deep, 12 inches deep—

Mr. OLNEY.—Q. That is, if the rainfall were just a foot, 12 inches?

A. Yes, if there was 12 inches of rainfall and it stayed so that it were 12 inches deep, you would have 50,000 cubic feet of water—I should say 50,000 lbs. of water, because each cubic foot weighs 62½ lbs. Now, the 50,000 lbs. of water represent 25 tons. If the rainfall were 24 inches, that weight of rainfall upon a bunker surface 20 by 40 feet would amount to 2 times 25 tons or 50 tons; so that the weight of water which falls upon a bunker area of 20 by 40 feet, when the rainfall amounts to 24 inches is 50 tons. **[1529—1466]**

Q. Now, just confining your illustration to that bunker of that particular area, just tell the jury how the computation would be made by which you determine the increase in the weight of the coal, or the percentage of increase in the weight of the coal passing through the bunker in the course of the year.

A. You have made the statement that the sum total of coal exposed at any one time—that is, assuming that 33,000 tons of coal are in storage, 32,000 tons of coal are in storage, and that 6,000 should be deducted for various reasons, the reason being that they are not exposed to the atmospheric condition, 25,000 tons of coal would weigh, assuming that there are 40 square feet to the ton, 1,000,000 lbs.; now, 1,000,000 lbs. of coal, if it were exposed one foot in depth, would have an area of 1,000,000 square feet; but we will assume a depth for the coal which would

(Testimony of S. W. Parr.)

be a reasonable average of the coal as it is piled; for example, it would be quite unusual for a storage pile to exceed 25 or 30 feet in height, the depth, however, if such coal were piled evenly, would be, according to the mathematical rule, one third of that height, or about 7 or 8 feet in height; now, then, we have therefore 1,000,000 square feet of area if our coal were only piled one foot in depth, one-seventh or one-eighth as great as if it were only piled one foot in depth; this then would reduce the area upon which the water could fall to about 100,000 square feet, that is, if it were 10 feet in depth, or we will say to about 120,000 square feet if it were a less depth; now, then, we have already seen that an area 800 square feet delivers a weight of 50 tons; how many such areas are involved in this area that we are talking about, that is, 100,000 square feet; **[1530—1467]** there are 125 such areas, each one taking 50 tons of water, which would give us in one year 6,250 tons of water falling upon that area. Now, if it were seven years in duration, the amount of water falling upon that area would be 43,750 tons. Now, we should assume that there must be some allowance made for evaporation possibly—certainly,—and other causes, so that it would be fair to drop down a reasonable amount for such losses, let us say one-fourth even, or a fifth it seems to me would be very conservative.

Mr. SULLIVAN.—Q. Deduction?

A. Deduction. Now, if you deduct a fifth of 43,750 tons for such losses you have a reduction amounting to 8,000 tons, and subtracting that from

(Testimony of S. W. Parr.)

the 43,750, it would give us we will say 35,800 tons of water in seven years. Now, if we divide this by the 2,000,000 tons in storage, to get at the percentage, we will have very nearly 1.8 per cent. Now, that is based on a 10-foot depth. If the pile were 7 feet in depth the percentage would be ten-sevenths of that amount, which would be 2 and a fraction, 2½ per cent as the average amount of rainfall that would be discharged on a moving quantity of coal having at any one time an area such as has been given and having a rainfall of the estimated amount.

Q. That is, 25 inches of rainfall?

A. 25 inches of rainfall; but I have estimated, or I would say that the allowance of one-fifth thrown out would perhaps make up for a small difference in the amount of rainfall, which is somewhat over the actual rainfall average.

Mr. OLNEY.—Q. What is the actual rainfall average that you found?

A. I think it is 22½ inches.

Mr. SULLIVAN.—Q. You made an allowance for 25 inches? **[1531—1468]**

A. My calculation was based on 25 inches.

Q. That is the Illinois rate of rainfall, is it not?

A. 40 inches in Illinois; 35 to 40, it varies.

Mr. OLNEY.—Q. Now, I want to get the essential elements of the process that you followed, without regard to the particular figures, clear in the minds of the jury, Professor Parr: you have taken a bunker, for instance, 40 by 80, and by reference to weather tables you can get the amount of rain which fell on

(Testimony of S. W. Parr.)

that area in the course of a year, can you not?

A. I can.

Q. And you get that in inches? A. In inches.

Q. And you can immediately transform that into weight, because you know the weight of water per cubic foot, per unit of cubic measure? A. Yes.

Q. And then you will know the weight of the water that fell on that particular bunker having a particular area during the course of a year, or if more than once a year is involved, during the course of several years; is that correct? A. True.

Q. Then if you take the amount of coal which has passed through that bunker in that time and say that all of this coal, for instance, was in one bunker, a big bunker containing 2,000,000 tons—of course that is a physical impossibility, but assuming it theoretically—then you would have a weight of coal of 2,000,000 tons passing through the bunker and having been exposed to a rainfall during that time computed in the manner indicated and taking the weight of the water and the weight of the coal as it passed through, you would get a percentage which would be theoretically the absolute gross amount to which the coal would have been increased **[1532—1469]** or could have been increased by reason of the rainfall: is that correct? A. That is true.

Q. And then you make a deduction from that because that of course is the theoretical perfect percentage, a reasonable deduction, in your opinion, to get the actual percentage? A. Yes, sir.

Q. Is that the process that you followed in the es-

(Testimony of S. W. Parr.)

sentials? A. That is the process.

Q. Would there be any difference between the deduction to be made for loss of water; that is, for the coal losing water, water draining off, in such a case as that where the coal is passing along and the deduction which you would have to make for loss of water, water draining off, where the coal did not pass along, and you were dealing simply with one pile or one bunker full of coal, the coal in which did not change during the year, for instance?

A. The fact that the coal is passing through the bunker would not be any reason why it should not take the full amount of water that falls upon it and retains it. If the pile stood in storage long enough to receive a saturation beyond its limit such a pile would lose the excess water doubtless by drainage.

Q. Now, let us take a practical illustration of that: suppose you had a pile of coal that was spread out so that it was only one foot in depth, that is, it was very shallow, and covered a large area, and it was exposed in this climate for six months, for instance, and six months during the winter, and there came, as we frequently have here, a rainfall of perhaps 5 inches in a couple of weeks, and that coal was not changed in that time, it remained the same coal, spread out to a depth only of one foot, would or would not that coal be **[1533—1470]** brought up to a point of saturation? A. It would.

Q. And the water would run off? A. It would.

Q. Now, suppose instead of the coal remaining there, the coal is passing along during this period, so

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that this lot of coal gets a part of the 5 inches, and this lot of coal gets another part, and this lot of coal gets another part, as it is passing along (illustrating), what would be the case there?

A. There would be no loss by drainage providing the coal moved sufficiently rapid to prevent an accumulation of moisture beyond the saturation point.

Q. In other words, putting it in other words, the result is that in the movement of the coal the coal is not so apt by reason of any one rain or any one wetting to be wet to such a point as to be saturated and to lose a considerable amount by drainage, while on the other hand if the coal remained still that is apt to take place and a considerable amount be lost?

A. The retention is in favor of the moving mass of coal.

Q. You were examined by Mr. Sullivan as to moisture content of screenings and lump at the mine; I don't remember now exactly what the question was, but will you state to the jury whether or not there is any difference in the moisture content of screenings and of lump coal as they come from the mine?

A. The coal in the vein has the water distributed evenly throughout the vein. When coal is broken out and some of it is ground up in the process and becomes fine material **[1534—1471]** it has still the same content of moisture that the entire mass had when it was broken out.

The COURT.—Q. There is no fine material as such in the mine?

A. There is not. It is all solid material. The

(Testimony of S. W. Parr.)

blasting of the coal tends to shatter a large amount, a comparatively large amount; the shoveling out and all those processes tend to produce a very considerable amount of fine material, but this process and the resulting products of fine and coarse at the time do not differ in the water content.

Mr. OLNEY.—Q. That is, if you take a case of one blast in the mine, a blast is put in and it breaks out the coal; now, some of that is lump coal and some of it is fine coal in the very process of breaking it out? A. It is.

Q. In the very nature of things? A. It is.

Q. And both the fine and the lump coal have the same moisture content? A. They do.

Q. Then the difference in moisture content between screenings and lump coal is a difference that is due to other conditions or to an accession or decrease in moisture after the coal leaves the mine?

A. It is.

Q. On that point which is the most subject or which is the most apt to take moisture and retain it?

A. The fine coal.

Q. And it is there that the difference lies between lump and screenings or fine coal as to a difference in moisture content? A. It is.

Q. Now, this may have been sufficiently covered, but I am going to ask the question anyhow so as to be sure of it: will you explain to the jury again what the significance **[1535—1472]** is of the differences in the percentages of moisture in the same coals as delivered at different times, in the tables that were

(Testimony of S. W. Parr.)

shown to you in Bulletin 41 of the United States Bureau of Mines?

* * * * * * * * *

A. The Bulletin to which you refer gave extremes of moisture content and the extreme figures were read by me; the question did not ask as to the water content of the coal as it comes from the mine, because a Bulletin just prepared by the United States Bureau of Mines, with upwards of 5,000 determinations, shows the inherent water content of the coal, and that was not the point as I understood the question which was sought to be brought out. The question was what under the ordinary commercial conditions are the excessive increases in weight of coal; now, there is a difference, as I understand it, between an excessive increase and an unusual increase; if there had been only one case of this sort, it would not have been fair to read it, but there were the common occurrences in the Bulletin. I think 10 or a dozen or possibly 15 cases were read; that is to say, under ordinary common commercial conditions what are the most unusual excesses in weight that may be met with due to the addition of moisture?

Mr. OLNEY.—Q. Now, Professor, getting at it in a little different way, at what point in the course of the handling of the coal were these samples taken, the results of which appear in those tables?

A. All of the Government data is taken on the coal at delivery at the point of consumption. **[1536—1473]**

Q. Then these tables were of samples taken upon

(Testimony of S. W. Parr.)

delivery of the coal to the Government at the point of consumption? A. They did.

Q. And these tables showed the variations in weight of the same coal as delivered to the consumer —namely, the Government, at different times?

A. They did.

Q. Now, the moisture content of this same coal at the mine is fairly constant, is it not? A. It is.

Q. Then the significance of these variations is that it shows the range within which the same coal may vary in weight due to variations in the moisture content? A. That is the case.

Q. After it is removed from the mine, and in the process of handling and sale? A. That is true.

* * * * * * * * *

Q. What did you do to test the samples which you had to see whether or not the coal was susceptible to oxidation?

A. That work was outlined in Professor Folsom's Laboratory and it consisted in putting the coals under test which would show the amount of oxidation they would undergo at given periods of time.

Q. What relation is there between the susceptibility of coal to oxidation and the amount of sulphur that is in it?

* * * * * * * * *

A. Do you refer to water that is in coal, inherent in the coal?

Q. No, I am asking now about oxidation, and asking you if there is any relation, and if so what that relation is, between the susceptibility of the coal to

(Testimony of S. W. Parr.)
oxidation and **[1537—1474]** the amount of sulphur that is in it?

A. The more sulphur the more—if I understand your question right—

Q. (Intg.) Does all contain some sulphur?

A. It does.

Q. Now, this is the point I am coming to, is there any relation between the susceptibility of coal to oxidation and the fineness of the particles of sulphur which are found in the coal? A. There is.

Q. What is that relation?

A. The more fines in which there is sulphur the more susceptible it is to oxidation.

Q. I don't refer to coal fines, but I refer to the fineness of the particles of sulphur themselves?

A. The finer they are the more easily they are oxidized and the more rapidly they are oxidized.

Q. You were cross-examined this morning, Professor Parr, with relation to certain tables which appear in Bulletin 28 of the Bureau of Mines; since the recess have you read that portion of the Bulletin which is concerned with those tables? A. I have.

Q. Take for instance the table on page 20: you were examined by Mr. Sullivan in regard to percentages shown under change in weight; that is, shown in the column under the heading "Change in weight," and you testified this morning as if those were changes in weight due to the process of oxidation; what is the fact in that connection?

A. I find that the water in the coal is less in amount and that is to be taken in in conjunction with the

(Testimony of S. W. Parr.)

change in weight in these particular cases.

Q. For instance, you were asked about this Illinois coal No. 9a? A. Yes. **[1538—1475]**

Q. The table shows an increase in weight of 0.17 of one % in four days; 0.47 of one per cent in 20 days, and a total increase in weight of 0.96 of one per cent in 387 days; now, is that the increase in weight which is due to the process of oxidation or is it the net increase in the weight of the coal?

A. It is the net increase and does not relate to oxidation.

Q. Immediately by the side of this last column to which I have referred there is another column headed "Moisture determination," that column shows a loss, does it not, of moisture determination during this time of 0.72 of one per cent? A. It does.

Q. And in order to get the total increase in weight due to the effect of the oxidation is it or is it not necessary to add the 0.96 of one per cent of increase in net weight and the 0.72 of one per cent of loss of moisture? A. That would be true.

Q. Does the author of the Bulletin do that very thing in the table on page 22?

A. That is the purpose of the table on page 22.

Q. And does it show there under the head "increase in weight due to oxidation" the 1.68% as the increase in weight in this very case?

A. That is the case.

Q. And is that true of the other coals that are examined *them?* A. It is, all of the other coals.

Q. That is, the table on page 22 is a table showing

(Testimony of S. W. Parr.)

the increase in weight due to oxidation; it is so headed, is it not?

A. Increase in weight due to oxidation.

Q. As compiled from the tables shown on pages 20 and **[1539—1476]** 21? A. That is true.

Q. Now, coming to this one particular thing that you were asked about in this Illinois No. 9a, in a period of 124 days, right at the end of the table there *there* appears a loss of minus 0.33 of one per cent. Now, I will ask you—

A. Excuse me, but what table do you refer to?

Q. The table on page 20, the first sample, and at the end of the column it shows a loss, that is, minus 0.33 of one per cent in change of weight in the last 124 days. Now, does that mean that through the process of oxidation the coal was losing weight during that time?

A. It does not. There was a constant increase at each period. This minus quantity here is due to the fact that there was a slight loss of water from the sample at the time, but there was an increase due to oxidation.

Q. As the process of oxidation goes on does there come a time when as the result of that process the coal loses weight, provided, of course, that the coal does not heat to such an extent as to practically burn.

A. There is no decrease in the process of oxidation—

Q. That is, no decrease in weight.

A. In weight; there may be an increase or a decrease in weight due to other causes but the process

(Testimony of S. W. Parr.)

of oxidation is a plus process always, within the limits of the high temperature, not too high a temperature to drive off the volatile constituents.

Mr. OLNEY.—Now, if the Court please, I am going to ask leave to read from the Bulletin upon this subject because there are some portions of it which throw light upon **[1540—1477]** this very matter. I think it is fair to go before the jury right now.

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Mr. OLNEY.—This portion of the Bulletin I desire to read to you, gentlemen, is as follows:

"Alteration of Weight of Samples of Coal When Kept in a Finely Powdered State.

"In order to investigate the question of the extent of alteration of the samples when kept in a finely powdered condition as prepared for analysis, portions of a number of such samples were put in weighed bottles, which were securely closed with rubber stoppers. These bottles were kept in the laboratory and weighed from time to time. Moisture determinations were made on portions of the sample at the times of the weighings, allowance being made for the portions removed for this purpose. The following gives the percentage of moisture originally present in the sample, the percentage of gain or loss in weight at the several weighings, and the time interval between the weighings; also the total time covered by the experiment."

Then following a set of tables which are the first tables appearing and which have these percentages of 0.17 and 0.47 and minus .33.

(Testimony of S. W. Parr.)

Mr. ROCHE.—You will concede Mr. Olney, that the weights there show the weights of samples of coal in that first table taken at different times.

Mr. OLNEY.—They show the changes in weight, not the weight.

Mr. ROCHE.—They show the changes in weight of the samples taken. **[1541—1478]**

Mr. OLNEY.—Yes, that is what it says, and that is what it shows, changes in weight.

"Without exception these samples all increased in weight upon standing. At the same time the moisture values usually decreased. The gain in weight is to be ascribed to oxidation, and the decrease in moisture either to actual loss or to fixation of a portion of the moisture present by the oxidation changes. If the moisture loss be considered an as actual escape of moisture from the sample, the total gain due to oxidation is equal to the observed gain plus an amount equal to this moisture loss. The table below gives the total oxidation changes considered on this basis, together with the original and final calorimeter determinations on some of the samples, also the loss in calorific value in excess of that due merely to changes in weight of the sample. For purposes of comparison the amounts of moisture, ash, and sulphur present in the sample are also given."

Then follows a table giving the laboratory number of the sample, the field number and the increase in weight due to oxidation, and running 1.68%, 1.78%, 2.47%, and finally a Wyoming coal 2.56% as the maximum, while the minimum increase in weight

(Testimony of S. W. Parr.)

is 0.40 of one per cent.

Then the Bulletin goes on:

"The oxidation changes in every case are sufficiently large to be of practical importance—the smallest change—that in the Ohio No. 5 sample—being 0.53 per cent,"—I don't understand why it says that because it appears here that the West Virginia is 0.41 per cent.

* * * * * * * *

Mr. OLNEY.—(Reading:) "The oxidation changes in every **[1542—1479]** case are sufficiently large to be of practical importance, the smallest change—that in the Ohio No. 5 sample—being 0.53 per cent, while the Illinois No. 9B sample showed a change of 2.47 per cent. The Wyoming No. 3 sample showed an increase of 2.56 per cent in weight and a decrease of 205 calories in heating value. The changes in weight correspond to a decrease in heating value of 144 calories, the final calorific value obtained being 75 calories lower than is accounted for by the changes in weight. Further results along these lines are desirable, but the values already obtained show very clearly that old samples of coal can not be regarded as representative of the original coal in composition or in calorific value."

Q. I will ask you to read to the jury the sentence prior to this Table 3, on page 18, about which you have been examined, and introducing that table from your report, just that sentence, there.

A. "Further data along this line are necessary,

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especially with large samples under actual storage conditions before any conclusions as to either an increase or decrease in weight would be warranted," referring to oven-dry samples, and not to moisture changes.

Q. Now, I will ask you to refer to the next table, which immediately follows Table 4, and ask if there does not appear a table of weights obtained by experiments conducted in the same manner, or in much the same manner, and showing increases in weight.

A. There is such a table and the basis of the reference is the same as before. All of these increases are gains in weight, or all of these figures are gains in weight. **[1543—1480]**

Q. And you were shown, on the one hand, tables showing decreases in weight, and on the other hand, tables showing increases in weight, where the experiments were practically the same.

A. The conditions of the experiments were not the same; I do not recall the second condition, but they were based on the same condition as to the matter of oven-dried or dry coal.

Q. You were simply presenting here, were you not, the various data you had gathered from time to time upon the subject? A. Yes, upon the subject.

Q. And some data might point one way and some another.

A. Yes, sir, and it was for that reason that the statement was made that further experiments in that connection should be carried on.

Q. As a matter of fact, has the knowledge of the

(Testimony of S. W. Parr.)

profession upon this subject of oxidation materially increased since this article was written?

A. It certainly has.

Q. And what has been the result of that increase of knowledge?

A. It shows that there is a uniform increase in weight, due to oxidation.

Q. Now, I will refer you to this portion of the article, a part of which was read to you:

"An editorial in Power gives Heidepin credit for showing that spontaneous ignition and weathering are due to direct oxidation of the coal substance and to the oxidation of pyrites, as Leibig supposed."

Pyrites is a combination of sulphur, is it not?

A. Yes.

Q. (Continuing.) "Bunte also credits the **[1544—1481]** phenomena to a direct oxidation and absorption of oxygen by the coal. The smaller sizes and the most porous coal are said to be affected the most. The absorption of oxygen may result in a gain in weight of as much as four per cent. W. A. Powers, chief chemist of the Santa Fe Railroad, in 1907, carried out an investigation of the weathering losses of the coals used on that road. These coals covered a wide range of county, samples being tested from Illinois, Missouri, Kansas, Colorado and New Mexico. One hundred-pound lots of coal were stored in the open air and under water for a period of seven months. The coal stored under water is said to have lost from .26 per cent to 5.92 per cent in weight, and from .56 per cent to 8.75 per cent in calorific value.

(Testimony of S. W. Parr.)

The coal stored in the open air lost in weight .60 per cent to 4.78 per cent and 1,10 per cent to 9.40 per cent in calorific value." Now, I will ask you if the changes in weight that are there referred to are changes in weight after the moisture factor has been eliminated, or with the moisture factor present?

A. They are based upon the water factor eliminated, and do not in any sense refer to the mass of coal as it was in storage. The same conditions prevail there as in the other case. Some of the coal was stored under water, as I recall it.

Q. It was stored under water, the bulletin says so. Now, this was read to you as a part of your conclusions in connection with this matter:

"Coal of the type found in Illinois and neighboring states is not affected seriously during storage when only the changes in weight and heat power are considered; the **[1545—1482]** changes in heat may be either gains or losses, and are probably never over 2 per cent in a period of one year. The heating power decreases most rapidly the first week after mining, and continues to decrease more slowly for an indefinite time."

I will ask you there, in that connection, if the changes in weight which are referred to are the changes in weight with the moisture factor eliminated, or with it present.

A. With the factor eliminated, on the oven-dry condition. That is the uniform condition where a variable is present.

Q. Were you, or were you not, dealing with coal

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that was stored under water? A. I was.

Q. So that it would be a saturated coal?

A. It would be; that sample was saturated.

Q. And were you, or were you not, putting on the same basis of comparison, coal which was stored in the open air, coal which was stored under cover, and coal which had been stored under water, so as to be saturated? A. Those were the conditions.

Q. And you were putting them all on the same condition.

A. On the oven-dry condition, without any water in them.

Q. You have testified that the process of oxidation goes on in two ways, one by action with the sulphur content, and the other by action with the carbonaceous content. A. I did.

Q. Which of those processes is the most active, or which will result in the greater increase in weight?

[1546—1483]

A. The latter phase of the question is altogether different from the other. Oxidation of the carbonaceous matter may be more active and produce less increase in weight. The oxidation of sulphur increases the weight by from 5 to 6 times the amount of the matter of chemical activity, and the result in weight will not have any relation to each other at all, but the ultimate increase is far greater in the oxidation of sulphur than it is in the oxidation of carbonaceous matter.

Q. We are dealing here with changes in weight purely and simply. If I get your answer correctly,

(Testimony of S. W. Parr.)

it is that of these two processes of oxidation which go on in the coal, the oxidation of the sulphur is the one which chiefly is responsible for the changes in weight, due to oxidation. A. It is.

Q. Now, by the oxidation of the carbonaceous material, you mean what we may call the ordinary coal constituents? A. Yes.

Q. The heat constituents of the coal. A. Yes.

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Q. Now, Professor Parr, you were examined this morning upon the tables shown on page 18 of Bulletin 38 of the University of Illinois, entitled the "Weathering Coal" of which you are an author together with W. F. Wheeler. This table is headed "Change in weight of coal exposed to the air," and it appears that it was of screenings, and in the case of screenings from Williamson County, Illinois, they were exposed in an open box on the roof, in one case, and in a barrel in a building in another case, that it was weighed December 15, 1908, and reweighed June 17, 1909, and that it had lost during that period 4.8 lbs., or 1.54 per cent **[1547—1484]** in one case, and two-tenths of a lb. or 0.07 of one per cent in another case. Now, I will ask you if these changes in weight which are given here in the table are changes in weight of that coal as it was put on the building, at the time it was put on top of the building, or put in the barrel in the building, and at the time it was taken out? A. Not at all.

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Mr. OLNEY.—Q. Will you call attention to these

(Testimony of S. W. Parr.)

weights and just state what these weights are?

A. These are weights which refer to coal in the dried state, after they had been heated in an oven at a steam temperature for one hour, and are weights of coal dried at 105 degrees.

Q. Is that fact shown on the face of that table, that that coal was heated, in both cases, to 105 degrees centigrade? A. It is.

Q. Would then any changes in weight due to a change in moisture content appear in that table?

A. Not at all; that was not the basis upon which this comparison was made.

Q. Why was that coal heated to 105 degrees centigrade before each of those weighings?

A. Because the coal was put under different conditions, as for example, one sample was submerged in a barrel of water and we could make no comparison as to change in weight of the mass, which was not the object of the table, hence they had to be brought to this condition which is described.

Q. Was not the heating of the coal to this high **[1548—1485]** temperature, just above boiling point, done for the very purpose of eliminating any factor of moisture content in connection with the experiments which were being made?

A. That was the purpose.

Q. Now, you were referred to this portion of this Bulletin which I am going to read to you:

"Storage Conditions.

"Object of the Experiments.—The object of these experiments was to determine the change in weight,

(Testimony of S. W. Parr.)

the change in calorific value and the amount of disintegration that are liable to occur in the grades of coal found in Illinois and neighboring states under different conditions of storage, (1) in the open air in piles; (2) in covered bins; and (3) under water."

To what changes in weight were references there made, or rather, to changes in weight under what conditions was reference there made when you spoke of changes of weight?

A. Not to the gross weight but to changes in weight which would be present without regard to any moisture content, but based upon the oven-dried sample.

Q. And why is that necessarily so?

A. We could not make any comparison between the samples as you have read them there, one being submerged in water and others being out in the open air. It was for the purposes of comparison.

Recross-examination by Mr. SULLIVAN.

In the tests which are illustrated on **[1549—1486]** page 18, table 3, of my Bulletin "Weathering of Coal," the coal was heated to 221 degrees Fahrenheit, that is, 5 degrees above the boiling point. The purpose of drying the coal out at that degree is to drive out the moisture. The sample of the screenings on the roof which was dried out at 105 degrees centigrade and all the moisture was taken out of it by the intense heat. The sample was weighed originally December 15, 1908, and then weighed again June 17, 1909.

Q. Now, then, at that time there was no moisture

(Testimony of S. W. Parr.)

in that sample, was there? A. There was not.

Q. And six months had elapsed between those two weighings, that is, from December 15, 1908, to June 17, 1909? A. Yes.

Q. The difference in pounds, according to your table, here, was 4.8 pounds, was it not? A. Yes.

Q. And the change in weight is represented by minus 1.54 per cent?

A. In the condition of absolute dry coal, without moisture.

Q. Now, that shows that by reason of oxidation, alone, regardless of moisture—because there was no moisture—there was a decrease in weight equivalent to 1.54 per cent; is that not so?

A. When referred to the moisture free condition. It has no reference whatever to the condition of the gross sample on June 17, 1909, nor to the condition of the gross sample on December 15, 1908.

Q. Does not that show the decrease resulting from oxidation, alone? A. It does not.

Q. What does it refer to?

A. It refers to the ultimate condition of the test, with all of the **[1550—1487]** plus and minus factors entering into it.

Q. Speak so the jury can understand you, please, and answer that question again. What does the 1.54 per cent refer to, what does it mean?

A. It means that the coal in the oven-dry condition, if all the mass of coal could have been oven-dried, weighed less than the same condition 6 months previous, if the coal was brought to the same condition.

(Testimony of S. W. Parr.)

Q. What brought about that change?

A. If we were to go into all the details of the experiment, as to what might make a difference there, you will recall that this test was carried out in a wooden box, and the product—the resulting effects of the experiment would carry into the wood a constituent which would not belong to the wood, but which normally would belong to the coal. Now, there are a number of conditions of that sort.

Q. Oh, do you mean that some of this increase of weight occurred by reason of the condition of the particles of wood in the wooden box?

A. No, I didn't say that.

Q. Now, please give another explanation, if you can; I would like to understand the explanation.

A. The condition here is that of the coal dried out at 105 degrees, and has no relation to the gross sample of coal at the end of the experiment.

Q. What does this 1.54 mean? Will you explain it please—minus 1.54?

A. It means that there was a seeming decrease in the mass of coal.

Q. A seeming decrease in the mass of coal which was free from moisture?

A. Which was free from moisture. **[1551—1488]**

Q. Which was free from moisture when it was first weighed, and which was free from moisture when it was last weighed? A. That is true.

Q. Now, during that period of 6 months, did the coal oxidize? A. It did.

Q. Notwithstanding the oxidation, there is a seem-

(Testimony of S. W. Parr.)

ing decrease in the weight of the coal, equivalent to 1.54 per cent. A. There is.

Q. After all the moisture had been deducted .

A. There is.

The COURT.—He is simply asked to explain what that percentage represents. Whether it is set down in the book, or is not set down in the book, he can tell what it means, if he knows.

A. It represents the loss of weight, under the conditions of that experiment.

The quantity of coal in that experiment was 311 pounds which would require a capacity of approximately 2 by 3 by 1 foot in depth; this was not a usual test; it was the only test that I know of that sort.

Mr. SULLIVAN.—Q. Assuming that 25 inches of rainfall on a box of coal 2 by 3 by 1, what would be the total weight of that box of coal and the water at the end of a year?

A. You mean the weight of the water that would fall on that area.

Q. Yes, in a box 2 by 3 by 1 foot.

A. It doesn't make any difference how deep it is, the area will catch the water.

Q. Well, say 2 by 3, and assume 25 inches of water fell on it.

A. 780 pounds would be the total amount of precipitation on that amount of area when there was a rainfall of 25 inches. **[1552—1489]**

Q. How many pounds, do you say?

A. 780 pounds.

(Testimony of S. W. Parr.)

Q. Now, let us deduct 1/5 of that weight for drainage and evaporation.

A. Then you must assume that the box is a sieve, and that—

Q. (Intg.) Just make the calculation, please.

A. The total amount of rainfall on that area, being 24 inches, would give a weight of 624 pounds.

Q. And plus the 300 would be 1000 pounds that the contents would weigh, allowing 1/5 for evaporation and drainage.

A. Excuse me, I don't think that is a fair statement.

The COURT.—Q. That is assuming it retained all the water in the box.

A. Assuming that the box could hold all the water.

Mr. SULLIVAN.—Q. Assuming by some act of Providence the water would remain right above that square box, assuming that a miracle took place, Professor, that there was a solid wall of water above this box 2 by 3 by 1.

A. Frozen into a mass of ice, for instance.

Q. Now, assume that in the last 50 years we had a rainfall of 25 inches of water per year here in San Francisco, how much would San Francisco be submerged after deducting 1/5 for evaporation and for drainage? Wouldn't we be submerged under a great mountain of water 84 feet high?

A. If you had no sewer system here, and allowing that there were no drainage systems at all.

Q. And allowing for evaporation and drainage, wouldn't we be submerged under about 84 feet of water?

(Testimony of S. W. Parr.)

A. I would have to figure it out. **[1553—1490]**

Q. I figured it out. Take it 25 by 50, that would be 1250, and dividing that by 12 it would leave 104, and then deducting 1/5 for evaporation and drainage, it would leave about 84 feet, would it not? Allowing 1/5 for drainage and for evaporation, we would be submerged under a wall of water of 84 feet in San Francisco, if we had had that rainfall for the last 50 years.

A. I do not understand that that implies that the calculation is incorrect. You assume that this calculation is all right.

* * * * * * * *

Q. In your book, Professor, there was something said about making tests of coals which were submerged in water; that was one of the experiments made by Mr. Powers, of the Santa Fe; how was the coal submerged in water for the purpose of testing?

A. Put in a barrel and enough water put in to cover the coal.

Q. It was just saturated with water to the point of saturation or moistened with water to the point of saturation.

A. No, the barrel was filled with water, so that it covered the coal completely.

Q. And, according to Powers, here, the coal that he tested, he allowed to remain in that condition for about 7 months; it appears from his report that the coal stored in water under those circumstances lost from 0.26 of 1 per cent to 5.92 per cent in weight, and from 0.56 of 1 per cent to 8.75 per cent in calorific

(Testimony of S. W. Parr.)

value. Now, the water did not do any damage to that coal, did it; it did not increase the weight?

A. The decrease in calorific **[1554—1491]** value which would indicate the damage, if you so call it.

Q. But, so far as weight is concerned, it did not increase the weight.

A. Those figures are referred again to the oven-dried condition of the coal.

Q. That is, after it was taken out of the barrel of water, a certain part of it is subjected to the test of heat, that is, 105 degrees centigrade, as I take it, and the moisture driven out, and then a sample is taken.

A. Yes, sir.

Q. That proves, does it not, that the water, itself, had no effect whatever on causing oxidation of the coal to an extent which increased its weight; is that not the fact?

A. The access of oxygen to the coal that is under water, I don't understand what your case would be.

Q. Does not the oxygen from the water unite with the coal, causing oxidation?

A. The oxygen in the water is already united and cannot oxidize the coal.

Q. I thought the humidity of the atmosphere tended to oxidize the coal; doesn't it?

A. It is the oxygen of the atmosphere that oxidizes the coal.

Q. Is it free oxygen in the atmosphere, or oxygen in the atmosphere of the air that combines with the coal?

A. It is the free oxygen of the air that does the work.

(Testimony of S. W. Parr.)

Q. Is it the free oxygen of the air that oxidizes steel, and iron, and metals, generally? A. It is.

Q. Or is it the oxygen in the water that causes the rust? A. It is the oxygen from the air.

Q. From the air? A. It is. [**1555—1492**]

Q. Will iron rust in a dry room, such as this?

A. It will.

Q. Will an iron or steel rule, placed upon this desk, and allowed to stay here for a hundred years, be affected by rust, if there is no moisture in the air?

A. The moisture facilitates the oxygen.

Q. Now, as a chemical fact, Professor, is it not the oxygen of the moisture in the air that combines with the elements in the coal and causes the oxidation of the coal?

A. I cannot, as a chemist, answer that question yes or no.

Mr. SULLIVAN.—Q. I will ask you this question: Is it not a fact that oxidation of steel and iron, and coal, and materials coming in contact with water, is caused by the union of the oxygen in the water with the iron, the coal, or the steel?

A. Steel would not rust in water if there were no free oxygen in the water. It would remain bright indefinitely if there were no free oxygen in the water.

Q. Will coal oxidize where there is no free oxygen in the air? A. It will not.

Q. Coal does oxidize when there is humidity.

A. There is free oxygen in the air, and it oxidizes; the humidity had nothing to do with the oxidation.

Q. You say the oxygen of humidity or the oxygen

(Testimony of S. W. Parr.)

of water has nothing at all to do with the oxidation of coal. A. It does not.

Q. Are you positive about that?

A. The free oxygen of the air is what does the work, just as it does in the case of the iron.

Q. How does the oxygen of water, or the moisture, rather, facilitates the oxidation? A. Of the iron.

Q. Yes, of the iron or of coal.

A. Because it carries oxygen with it. There is oxygen dissolved in the water, just as other substances dissolve in water, and that **[1556—1493]** is the agent that is active in oxidizing the iron.

Q. The water consists of oxygen and hydrogen in certain parts, and you say in addition to that there is free oxygen in solution, is that so? A. Yes, sir.

Q. And it is this free oxygen in solution that causes the oxidation of the metal.

A. The action—coal under water is not subject to the same action.

Q. Is there any oxidation of coal under water?

A. So far as we are able to determine, there does not seem to be any appreciable oxidation of coal when it is submerged under water.

Q. Is there any oxidation?

A. The experiments are entirely too meagre to base any definite statement on about it.

[Endorsed]: Filed Jan. 19, 1915. W. B. Maling, Clerk. By C. W. Calbreath, Deputy Clerk. **[1557—1494]**

[Testimony of E. E. Somermeier, for Defendants.]

E. E. SOMERMEIER, a witness called for the defendants, and sworn, testified as follows:

Direct Examination by Mr. OLNEY.

I am Professor of Metallurgy in the Ohio State University, and reside in Columbus, Ohio. I graduated from the afore-mentioned University with the class of 1898 and have been connected with the Department of Metallurgy there ever since that time. I began the study of coals in 1899 and have been interested therein practically ever since that time. I have made a specialty of that study. I was formerly associated in my studies and investigations in coal with N. W. Lord, formerly Professor of Metallurgy in the Ohio State University. He was the Chemical Director of the Coal Laboratory of the United States Geological Survey for a considerable period from 1904 on. At the time of his death he was consulting chemist of the United States Geological Coal Testing Laboratory at St. Louis, Missouri, and also of the United States Government laboratories at Pittsburgh, Pennsylvania, Washington, D. C., Norfolk, Virginia, and Columbus, Ohio. Professor Lord undoubtedly ranked as the foremost authority in America on coals. He was a pioneer in this study, especially from the modern standpoint in respect to determination of heating value and commercial utilization. His death occurred in 1911, when I succeeded to his position. I have in my study of coals been particularly concerned as a specialty with the determination of moisture, oxidation and heat-

(Testimony of E. E. Somermeier.)

ing value. I did the coal work for the Ohio Geological Survey from 1900 to 1907. The results **[1558—1495]** of that work appear in published form in Bulletin Number 9 of that survey, partly under my name and partly under Professor Lord's. Coal is the most important mining industry in Ohio. The annual production is upward of 20,000,000 tons. From May, 1904, to September, 1905, I had direct charge of the United States Government Laboratory at St. Louis, Missouri. That was the testing laboratory of the United States Geological Survey. After that I remained in a supervisory committee with respect to that laboratory until it was moved to Pittsburgh in January, 1907. Also, in the years 1906–1907 I was in charge of a branch laboratory of the United States Geological Survey at Columbus, Ohio. My assistants in that laboratory were Mr. Fieldner and Mr. Davis, who are at present Chief Chemists for the Coal Laboratories of the Bureau of Mines. In association with Professor Lord, I had consulting connection with the United States Laboratories at Pittsburgh, Norfolk, and Washingeon, and visited each of these places one or more times. The Norfolk Station was in connection with the Jamestown Exposition, just as the aforementioned St. Louis Station was in connection with the World's Fair Exposition. I have published a number of articles in the Journal of the American Chemical Society, and I am also the author of a text-book on coal. I have for various coal companies examined the coals in mines and passed on its quality. I have been engaged in the

(Testimony of E. E. Somermeier.)

commercial testing of coal with reference to the utilization of coal under steam boilers, that is to say, the conditions for obtaining the highest efficiency and best return for fuel. [1559—1496]

Q. Now, Professor Somermeier, to what extent if at all, does coal vary in weight from time to time as coal is handled commercially, and in the course of transportation from the mines and storage and sale?

A. It depends very largely upon the condition of the coal and the time in transit; it may vary anywhere from 1 to 10 or 15 per cent.

Q. Is that fact a fact well known to science?

A. Every man familiar with the coal business in the east is familiar with it.

Q. What are the particular causes of such changes in weight? A. Changes in moisture and oxidation.

Q. What is the practice among large coal consumers in the east, including the Government of the United States, in regard to purchasing coal upon specifications containing a provision as to the moisture content of the coal?

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A. The larger concerns in the east buy coal under specifications which recognize the fact that the moisture in the coal is simply an increase in weight without any increase in heating value.

Mr. OLNEY.—Q. Now, when coal is purchased under specifications containing a provision as to moisture content, what is the method followed for ascertaining whether the coal conforms to those specifications, in determining what allowance must

(Testimony of E. E. Somermeier.)

be made by the seller because of the moisture content of the coal?

A. The coal is sampled at the time it is weighed and analyzed, and the amount of moisture determined, and a settlement made upon that basis. [1560—1497]

Q. Now, how often would such samples be taken in the case of a delivery extending over a period say of days?

A. It should be taken with every delivery that admits of the expense of taking samples; in practice it is not possible, or is it not profitable at least to sample less than 50 ton deliveries.

Q. Suppose the coal is being delivered in deliveries of 50 tons each per day?

A. Each day's deliveries should be sampled.

Q. And the sample should be taken daily in this case? A. Yes.

Q. Why should the sample be taken daily?

A. Because the moisture content of the coal varies continually.

Q. It varies from day to day, does it? A. Yes.

Q. Now to what extent do these variations from delivery to delivery and from day to day in the moisture content correspond to changes of weight?

A. They are practically the same thing; a change in moisture means a change in weight, for all practical purposes.

Q. Now, are these variations in moisture content and in corresponding weight changes confined to differences between different coals or coals from dif-

(Testimony of E. E. Somermeier.)

ferent mines, or are they found in deliveries of coal of the same character from the same mine?

A. They are found in every coal, it does not matter from where it comes, the same mine or different mines, it shows changes in moisture.

Q. In that connection, take the coal from a particular mine, would there be any substantial variation in the **[1561—1498]** moisture content of that coal as mined regardless of whereabouts the coal was taken from?

A. As it occurs in the mine, the moisture content in the mine will run pretty near uniform from year to year; practically we might say that it is constant; you can say that for one mine the moisture will run about so and so; for another mine another value.

Q. Then these changes in the moisture content of coal and the corresponding changes in weight or variations in weight are variations which take place after the coal is broken out from the seam?

A. Yes.

Q. Are the facts in regard to the constancy, as I may say, of the moisture content of the coal from a given mine and the variability of the moisture content of the coal after it has come out of the mine facts well known to science?

A. Yes, the United States Government has sampled coal from hundreds and hundreds of mines, and has taken the utmost pains to have the mine sample show the moisture in the mine at the time, so that it might be of value for all time, showing what the moisture in that mine runs.

(Testimony of E. E. Somermeier.)

Q. Now, within what range are these variations in weight, variations in moisture content and corresponding variations in weight after the coal leaves the mine, observed in commercial practice?

A. Anywhere from 3, 4, 5, 6 or 7 per cent.

Q. Has the Government published a bulletin showing the result of analyses of coal deliveries to it?

A. Yes.

Q. That is published in Book 41, is it not?

A. Yes.

Q. Have you it there? A. I have a copy of it.

Q. Will you turn to the tables that are there **[1562—1499]** given showing such deliveries and read to the jury some of the variations in weight in the same coals which are there shown?

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A. On page 38 of that Bulletin the deliveries to the Baltimore, Maryland Customs, show moisture in October, 3.50; moisture in January, 8.40; moisture in February, 6.75.

Q. What is the extreme difference in variation that is shown there?

A. Well, the sample at Boston, Massachusetts.

Q. (Intg.) I mean in this particular table that you have reference to.

A. This particular table is about 4.9 per cent.

Q. Is all of that coal from the same mine, represented in that table?

A. It is marked as being the same kind of coal, anthracite screenings.

Q. That difference of 4.94 would indicate a change

(Testimony of E. E. Somermeier.)

in weight in that coal as delivered at different seasons of the year? A. Yes.

Q. Now, just pick out one or two more.

A. Boston, Massachusetts on page 39, delivery to the post office and Subtreasury, shows in October, 1.20; in January, 7.10; February, 4.90, a difference of 5.90. Brooklyn, New York—

Q. Now, in that case where you find a difference of 5.90 per cent is that a difference in regard to the same coal?

A. Yes, it is marked Pocahontas and New River coal. As mined that coal does not contain over 2 per cent, 1½ to 2 **[1563—1500]** per cent so that any increase above that would mean that the water had gotten in it, after leaving the mine.

Mr. ROCHE.—Q. Does that refer to screenings?

A. No this is the run of the mine coal.

Mr. OLNEY.—Q. Any other figures there?

A. Brooklyn, New York, Courthouse and Post-office, Anthracite, Buckwheat—

Q. Just leave out the case of the Anthracite and pick out the bituminous coal?

Mr. ROCHE.—The first illustrations from which the witness has read refer to Anthracite.

A. The first one is; the second one does not; the Pocahontas is not an anthracite coal. Buffalo, New York, October, on page 40, 2.40; January, 6.30; February, 4.20, an extreme difference of 3.9. This is Shawmut coal from Pennsylvania, a bituminous coal. Cincinnati, Ohio, on page 41, shows for October, 1.80, January, 4.05, February 3.15. This is New River

(Testimony of E. E. Somermeier.)

Smokeless coal from West Virginia; also a bituminous coal. Milwaukee, Wisconsin, October, 3.00, January, two shipments, one running at 5.30, and the other running 9.90; February, two shipments, one running, 7.85 and the other running 6.00%. Yougheogheny screenings, Pennsylvania; that is bituminous coal.

Q. Do you know what the percentage of that coal in the mine is?

A. The Yougheogheny coal?

Q. Yes.

A. It is a low moisture coal, 2 or 3 per cent; I don't know what it is exactly.

Q. What is the maximum percentage?

A. The maximum percentage here is 9.90, I think **[1564—1501]** I could get it for you.

(The tables above referred to were here stated to be in Bulletin 41, heretofore introduced in evidence in connection with the examination of Professor Parr, as Defendants' Exhibit "GG.")

Q. Now, Professor Somermeier, what is the significance of these large variations which I have asked you to call to the attention of the Jury—what do they show?

A. They show that the coal has increased in weight from the time it was taken from the mine until it was delivered at the various places.

Q. By those percentages? A. Yes.

Q. Now, by the way, will you glance through these tables and see if there is any difference in the percentages between seasons of the year?

(Testimony of E. E. Somermeier.)

A. These values I have already given you would be indicative of that, the winter season being the wet season in most of these places in the east.

Q. By the way, Professor, Somermeier, in connection with those variations are you acquainted with the Government method of sampling? A. Yes.

Q. And of making analyses? A. Yes.

Q. Did you have anything to do with the installation of the system, in fact?

A. Well, I might say I founded it.

Q. Now, in connection with these variations in weight or in connection with the analyses as published **[1565—1502]** in the Government reports for the purpose of getting the moisture content, is there a factor of error between the result as published and the actual moisture content?

A. Yes, there is, in a majority of cases.

Q. Which way is that factor?

A. Well, the results as published, wherever the moisture runs high, are low of the coal moisture that is in the coal.

Q. Why is that?

A. Because of the difficulty of avoiding losses during sampling down, where the coal contains much moisture.

Q. Is there the same difficulty in getting the correct moisture content where the moisture content is low?

A. No; if the sample contains very little moisture it cannot lose very much in sampling down, and there is very little difficulty with it. In the case of the

(Testimony of E. E. Somermeier.)

high moisture coals it takes just as long to sample them down and they can lose moisture 10 or 20 times as fast, hence the errors may be 10 or 20 times as large in the high moisture coal.

Q. Then in fact the variations in moisture content which actually exist are to some extent larger than those that appear in that published report?

A. Yes undobutedly.

Q. Have you made yourself familiar with the character of Wellington coal or Richmond coal or Australian coal and the Japanese coal received here?

A. Yes.

Q. As to whether it is anthracite or bituminous coal or lignite? A. They are all bituminous coals.

Q. I think you testified that while the coal from all parts of the mine would have about the same moisture content—I want to ask you a question in that connection: **[1566—1503]** will that moisture content vary as between mines?

A. That depends considerably upon the region; in regions where the coal is not too much disturbed, the same mine or different mines in the same region will run about the same moisture content; in other regions if the coal is much broken up by geological disturbances, different mines will differ quite a little in moisture content.

Q. In the same locality? A. Yes.

Q. Now, is there any difference, for instance, in the moisture content generally between the coal of West Virginia and the coal of Illinois?

A. Yes, there is a very large difference.

(Testimony of E. E. Somermeier.)

Q. What is the character of the coals from Illinois?

A. The coals of Illinois will contain anywhere from 12 to 15% moisture in the mine.

Q. And the West Virginia?

A. West Virginia, most of them run as low as 2 or 3%.

Q. Are coals generally divided with reference to their moisture content into high and low moisture coals?

A. Yes, for convenience they are usually classified as high moisture coals, medium moisture coals and low moisture coals.

Q. What is the range of low moisture coals?

A. Anything below 4 or 5 per cent.

Q. Now, have you made analyses of these Australian, Wellington and Japanese coals to ascertain the character of these coals as to moisture content?

A. I **[1567—1504]** have analyzed the Wellington and Australian. I failed to analyze the Japanese.

Q. What did you find the case to be with the Wellington and Australian coals?

A. They were low moisture coals, containing about 3 per cent of moisture.

Q. When you call it a low moisture coal, you have reference to the moisture content of the coal in the mine, have you not? A. Yes.

Q. Is there any difference between the behavior of a low moisture coal and that of a high moisture coal, after it has been taken from the mine and exposed to

(Testimony of E. E. Somermeier.)

the air and weather in the course of shipment and storage and handling?

A. There is a very marked difference. The low moisture coal, in coming from the mine, will lose only a small amount of moisture, because there is only a little moisture in the coal; a high moisture coal, such as Illinois, for instance, may lose ten per cent of moisture before it gets down to a condition of somewhat near equilibrium with the atmosphere.

Q. Take the case of a coal coming down here from Nanaimo, and by the way, Professor, have you visited Nanaimo? A. Yes, sir.

Q. Take the case of a coal coming down here from Nanaimo, the Wellington coal, the coal that is mined there, coming in a vessel under hatches, and it takes, say, four days to make the journey, what change, if any, in the moisture content of that coal will take place in the course of that voyage?

A. In my opinion, the change would be very small.

[1568—1505]

Q. Could you form an opinion as to whether that change would be an increase in weight, small as it may be, or a decrease in weight, small as it may be?

A. Yes, I could form an opinion as to that.

Q. What is it?

A. There very probably would be a very slight decrease.

Q. After that coal arrives here, and arrives here with a low moisture content, perhaps a little lower than the moisture content which it had when it was mined, what is going to be its behavior with regard

(Testimony of E. E. Somermeier.)
to moisture content?

A. As I stated before, the low moisture coals cannot lose very much moisture, and this San Francisco climate is high in humidity, averaging about 80, so that that coal after leaving the mine and coming into this climate, cannot lose, in my opinion, over .3 or .4 of a per cent; on the other hand, it can retain or gain practically all the rainfall that ever falls upon it, if it is subject to rainfall, so that it may increase anywhere from 1 to 7 or 8 per cent.

Q. If this cargo of coal that is shipped from Nanaimo here had been rained on in the course of being loaded and before it is weighed for shipment, would it lose .3 or .4 of a per cent by the time it arrived here, or gain, or would it lose more or gain more?

A. If it is wet up there at the loading point, it would stand a chance of losing considerable on the voyage.

Q. Are all the conditions of transporting coal under hatches from Nanaimo here, conditions that are favorable to a loss of weight, rather than a gain of weight?

A. Well practically all the conditions; of course, **[1569—1506]** even in that short journey, there is a condition that would favor a slight increase in weight, due to oxidation, but that is a minor effect, as compared with a condition that would tend to make a loss for that period of time.

Q. Now, as between a fine coal and lump coal, which will increase the more by reason of an accession of moisture, say of rain? A. Fine coal.

(Testimony of E. E. Somermeier.)

Q. What are the approximate limits to which lump coal will take on moisture and fine coal will take on moisture, that is, assuming you have a low moisture coal, such as we are dealing with here?

A. Lump coal will not take on over 1 or 2 per cent, while fine coal, depending upon the degree of fineness, may take as high as 10 or 15 per cent.

Q. Were you present when a test was made to ascertain the percentage of fines in a cargo of Australian coal that was landed here? A. Yes, sir.

Q. Just tell the jury what that experiment was, or test was?

A. The test was to determine the percentage of fines in the coal in the bunkers at Folsom Street, and was made by simply keeping track of the amount of coal and the amount of screen coal that came from coal in the ordinary course of business during, I believe the test extended two days, the amount of coal handled amounted to 700 tons, and the screenings amounted to about 24½ per cent, somewhere in that neighborhood, I have not the exact figure with me.

Q. Have you observed any other cargoes that were **[1570—1507]** discharged there, excepting that particular one?

A. I have observed a number of cargoes.

Q Did you observe cargoes coming in with greater percentage of screenings?

A. Yes; I should say that the test was, if anything, rather below the average in the amount of fines; and that some of the cargoes, from their appearance, would run 40 or 50 per cent fines.

(Testimony of E. E. Somermeier.)

Q. To what extent would a pile of screenings, or a pile of coal containing a large percentage of screenings, give up its moisture when it has once been thoroughly wet?

A. It would give it up, of course, from the surface, but if the pile is wet in the interior, it will give it up very slowly; after it dries out on the surface, and is apparently dry, if you disturb it down a few inches, you will find damp coal.

Q. Suppose a pile of coal, with a considerable portion of screenings, were wet, say in the winter, or in the spring, and stored out in the open, exposed to the sun, and a lot of it remained there for six months, in this San Francisco climate without rain, would that pile of screenings retain a substantial portion of its moisture?

A. I would say that it would retain the greater portion of its moisture; the drying out would be confined to the outer foot, or two, of the pile.

Q. Did you take any samples at Nanaimo to ascertain the behavior of the coal there? A. Yes, sir.

Q. Just tell the jury what you did?

A. I sampled the mine in two places, and I also obtained a sample of the lump coal which had been standing in a car under cover for **[1571—1508]** five or six weeks: I also sampled a car that had been standing on the track, a run of mine coal, for five or six weeks; I also took a sample of fine coal off the top of a car where it had been exposed to a drying out; at the time I took the sample, it was about four o'clock in the afternoon, and the sun had been quite

(Testimony of E. E. Somermeier.)

warm all day; I took the temperature and the humidity at that time, and the humidity only ran about 40 per cent, so that the sample was taken under what might be called extreme conditions of dry weather for that port.

Q. And you found that the moisture content of the sample taken from the mine was about what you have already testified? A. About 3 per cent, yes, sir.

Q. And what was the moisture content of the coal that was in a car under a shed, under a cover?

A. The moisture content of that sample was 2.54 per cent; the moisture content on the car sample which had been standing in the yards ran 3.16 per cent—no, I should correct my statement on that sample in the shed, that ran 2.75; the sample in the shed ran 2.75, the sample on the track 3.16, and the sample of fine, dry coal on top of the car ran 2.2.

Q. Did you take any other samples except these samples that you have testified to?

A. I took two mine samples. The mine sample from the hard coal ran 2.97, per cent, and the sample from the steaming coal seam ran 2.92 per cent.

Q. Did you take any samples here in San Francisco? A. Yes, sir.

Q. Of what coal, Professor Somermeier?

A. I sampled a bunker of Australian and a bunker sample of the screenings, and I also obtained a sample which had been collected by **[1572—1509]** Professor Folsom at the time the ship was unloaded. The sample from Professor Folsom ran 3.19 in moisture; the sample of the screenings ran 3.25 and the

(Testimony of E. E. Somermeier.)

sample of the bunker ran 3.2.

Q. That was the coal as delivered, was it?

A. As delivered.

Q. That is, as it came out of the vessel?

A. The sample taken by Professor Folsom was a sample taken out of the vessel.

Q. Do you know what vessel it came from, what was the character of the coal?

A. I only have the description from Professor Folsom, it was an Australian sample, and the sample was dried.

Q. Now, Professor Somermeier, will you explain to the jury what this process of oxidation is about which you have spoken?

A. As long as coal remains in the seam undisturbed and protected by a cover of rock and earth, it remains practically unchanged, but as soon as it is broken out of the seam and exposed to the air, it at once becomes subject to atmospheric agencies, and undergoes what is chemically called weathering. That can be noticed that the fact that the coal that comes out of the mine after awhile is noticeably duller in appearance at a fresh fracture, and occasionally you can notice in the crevices of the coal a gray or light colored powder. The dulling of the surface, and this light colored powder, are both the result of oxygen being taken up by the substances of the coal. The light-colored powder, or greenish in large masses, is the result of the action of the oxygen **[1573—1510]** upon the sulphur in the coal; the sulphur in the coal—it always occurs in coal, and usu-

(Testimony of E. E. Somermeier.)

ally occurs in what is chemically called sulphur balls or brassy suphur, or pyrite, or fool's gold—it has a number of names; it occurs sometimes in large masses, and sometimes in flakes so thin that you can hardly detect them with the naked eye. In the large masses, that sulphur does not oxidize to amount to anything, but wherever it occurs in thin films or flakes, it presents a large surface to the air, and you get a very appreciable oxidation, and that is what causes the appearance of this gray powder along the fracture of the fissure. The dulling, as I have said, is simply the taking up of oxygen by the carbon and hydrogen compounds of the coal. That is not so easily explained in complete chemical terms as the oxygen goes into the coal and is held there; some little bit of it escapes out again, carrying a very small amount of carbon; but the chemical experience is that the amount that goes off is only about one-quarter of the amount that comes in, so that the net increase in weight is about three to one. That ratio is never overbalanced until the coal gets warm enough to burn. Of course, when the coal burns, it all goes off as a gas, or vapor. But in ordinary temperature, the oxygen that goes into the coal is retained there as a loose chemical compound.

Q. Does this process increase the weight of the coal? A. Yes, sir.

Q. To what extent? I will ask the question this way: What is the highest limit in the way of change of weight which has been observed this process may be carried **[1574—1511]** to?

(Testimony of E. E. Somermeier.)

A. I don't know that I could give you the highest limit, but I have in mind limits as high as four or five per cent.

Q. What effect has an increase in temperature upon this process of oxidation? That is, assuming always you do not get up to the ignition point?

A. An increase in temperature accelerates the rate of reaction; an increase in temperature of about 30 degrees for example, will about double the rate of reaction; if you go up 30 degrees more it will double it again, so you cannot get so very far up until the reaction gets high enough to make heat, and the more heat the more temperature and finally the coal burns.

Q. By this reaction, you mean the process of taking up oxygen?

A. The process of taking up oxygen.

Q. Take the case of a cargo which was stored, say in January of one year, and allowed to remain there during the balance of that year, and then was found to have commenced to heat and finally discharged in June of the year following, that is to say, that it had been there a year and a half, the cargo in the meantime having been kept under cover, where the water could not get at it, and where the rain could not fall on it, would you expect any substantial increase in weight from oxidation?

A. Yes, I should expect an increase, in a situation of that kind, anywhere from 2 to 4 per cent; I do not know, in fact, where I would put the limit.

* * * * * * * * *

Mr. OLNEY.—Q. Professor Somermeier, now

(Testimony of E. E. Somermeier.)

take the case of a storage pile in the open air, of a very considerable [**1575—1512**] storage pile in the open air, of a very considerable size, which began to heat, and in which they ran pipe down into the center of the pile, and around to various places in the pile, and through those pipe, they let water run into the pile, in an endeavor to stop the heating, and practically flooded the pile with water in that way, would the process of oxidation go on rapidly in such a case?

A. Yes, sir, it would go on as a rule more rapidly than in the other case, because that part of the oxidation which is due to the sulphur is accelerated by the moisture. That part of the oxidation which takes place in the coal substance itself takes place in a dried coal as well as in a wet coal; but any decided oxidation of the sulphur in the wet sample would make the oxidation results higher in a wet sample than in a dry sample.

Q. Is the process of oxidation increased by repeated wettings, that is, one after another?

A. The general tendency would be to increase the process of oxidation, because it would insure a plentiful supply of water at all times to that sulphur that might be present.

Q. Now, take the case of Australian coal, for instance, such as you have examined, brought here, but not heated, that is to say, it does not become heated, but is allowed to remain in a pile, and is perfectly normal, nothing unusual about it, at all, how rapidly would this process of oxidation go on there?

(Testimony of E. E. Somermeier.)

A. Well, I should say that in the course of two months that sample might oxidize, even under those conditions, perhaps half a per cent.

Q. Would the humidity of the atmosphere, as distinct from rain, have any effect upon these coals brought **[1576—1513]** here, either the Australian or the Nanaimo coal?

A. It would have a very appreciable effect upon the Australian coals, because of the fact that they come in—the samples that I saw, or the cargoes that I saw—practically dust dry, and coming into this atmosphere a dry coal will take up moisture out of the air in very appreciable quantities.

Q. And what would those quantities be?

A. I have performed experiments in the laboratory which have run as high as 2 per cent with a change of humidity of 20, that is, a humidity running from 50 some to 70 some, but I would not believe that that would occur to that extent on a cargo.

Q. Will you give an estimate of within what limits that might take place?

A. I would not want to put the estimate over half a per cent, and probably less than half a per cent. Coal is very peculiar in that respect, in that it has a limited ability to take up moisture, but within that limit, it has a very strong affinity to take up moisture, and until it is satisfied it will take up very rapidly from substances which, themselves, hold moisture; that is so well known that in the laboratory, in the analyses of coal, in determining the moisture in the laboratory, the sample is heated in an oven for an

(Testimony of E. E. Somermeier.)

hour at 105 degrees centigrade, that is a little above the boiling temperature of water, and as soon as it is taken out of the oven, it is put in a closed container, containing calcium chloride, which is a chemical substance, and which has a great affinity for water, and it is allowed to stay in there until the sample is cool, and then it is weighed; the loss in weight is, of course, a figure that is called **[1577—1514]** moisture. If that sample is weighed promptly, the results are different than if the sample be allowed to remain over night, with that drying reagent as during the night that sample of coal will take up out of this dry reagent, appreciable quantities of moisture again; so that samples that might run 3 per cent of moisture loss if weighed promptly, if kept over night, in contact with that drying reagent, will take up enough moisture out of that drying reagent to increase its moisture loss to only 2 per cent; in other words, it will take up a per cent out of the desiccated reagent put in there and keep the moisture from going back into it. The chemist has to be very particular not to allow his chemical to lag, he must take it up very quickly; it cannot be allowed to remain over night; unless you have a stronger desiccated reagent than calcium chloride. I had occasion in my work with the Government, to do a very great amount of experimenting in that line, because the moisture determination is such an important determination, from a commercial point of view, and I proved conclusively that samples would run a per cent low if they were not weighed up the

(Testimony of E. E. Somermeier.)

same day they were started, unless concentrated sulphuric acid was the drying reagent; concentrated sulphuric acid will take up water so strong that even the coal cannot take it away. The calcium chloride, which is a government chemical dryer, has not an affinity strong enough to hold it.

* * * * * * * * *

Q. Have you familiarized yourself with the moisture records of the United States Meteorological Station here **[1578—1515]** in San Francisco?

A. Yes, sir, I have.

Q. And have you familiarized yourself with the humidity records, likewise? A. Yes, sir.

Q. Now, take the case of a firm importing into this port, and selling here during the course of years cargoes of coals such as those you have examined, that is, low moisture coals, and assuming for the purposes of your answer that the coals are discharged from the ships in which they are brought here into bunkers, barges and yards, and delivered from such barges, bunkers and yards and sold by the importer, would such coals increase or diminish in weight within the time of their importation and the time of their sale, bearing in *mine* the climatic conditions existing here? A. They would increase in weight.

Q. Are you familiar with the bunkers, and barges, and yards of the Western Fuel Company?

A. Yes, sir.

Q. Now, assume that the Western Fuel Company was engaged in the business of importing and selling coal during the years 1906 to 1912, inclusive, and that

(Testimony of E. E. Somermeier.)

during that period it imported into the port of San Francisco approximately 2,000,000 tons, that these coals were British Columbia, Japanese and Australian coals, in the proportion of about 70 per cent from British Columbia, 5 per cent from Japan, and 25 per cent from Australia, and were of the character of the coals you have examined here, and that in order to carry on its business, the Western Fuel Company kept in stock in its bunkers, including the Folsom Street bunkers and other bunkers, and in its barges, similar to those now in use by it, and in its yards, an average of 32,000 tons of these coals during the period mentioned, **[1579—1516]** and that of these 32,000 tons, not to exceed 7,000 tons was stored under cover, and that 25,000 tons was stored either in open bunkers or in barges, or in yard piles, what changes, in your opinion, did those coals undergo while in storage?

A. Changes in weight and oxidation, or, rather, changes in moisture and oxidation.

Q. Did those changes affect the weight of the coal?

A. Yes.

Q. Was the weight increased or decreased by those changes? A. The weight was increased.

Q. To what extent, in your opinion, was the weight increased?

* * * * * * * * *

A. From 2 to 4 per cent, I would say.

Q. Have you ever visited the barges when they were delivering coal to Pacific Mail liners?

A. Yes.

(Testimony of E. E. Somermeier.)

Q. And you know the character of the coal so delivered, that is to say, as to whether it was fine or coarse coal?

A. Yes, sir, I have a fair idea of it.

Q. Now, where such coal was so dry as to be dusty in the course of handling, what percentage in weight of water would be required to be added to it, in order to lay the dust?

*   *   *   *   *   *   *   *   *

A. Well, I would say that under those circumstances, if the coal was so dry as to require wetting down, it would take at least two per cent to wet it, and bearing in mind the practical conditions under which the water would have to be applied, that some of it would be **[1580—1517]** soaked much more than enough in order to insure that the other secured a sufficient amount to wet it, that there might be as much as 4 per cent added; and on the average, perhaps 3 per cent.

Mr. OLNEY.—Q. Now, if you assume that the coal delivered by the barges was wet down during half the year, that is, during the dry months, six months of the year, and that the amount delivered year by year, that is, each half year by half year, by these barges, was approximately the same, you would have from 2 to 4 per cent, or an average of 3 per cent, say, added to these particular barge loads which were wet, and on the average, extending over the whole year, you would have half of that, or 1½ per cent, would you not?   A. Yes.

Q. In getting the percentage to which the coal may

(Testimony of E. E. Somermeier.)

have been increased by the natural causes to which you have already testified, you would then, so far as the barges are concerned, assuming that half of them are wet down, add 1½ per cent to these percentages of from 2 to 4 per cent from the natural causes to which you have already testified.

A. Yes, sir, that would follow as a natural consequence.

Q. That would follow simply, of course, as to the barge coal. A. Yes.

Cross-examination by Mr. SULLIVAN.

I am at present engaged as an expert by the Western Fuel Company. I began my investigations for them last August, at the same time when Professor Parr began. We **[1581—1518]** were together at Nanaimo and we are here together at San Francisco, but we have carried on our chemical investigations at different places. We carried on correspondence with one another during the period of time that each of us was carrying on these investigations.

I received a copy of a report that Professor Parr had sent to the counsel for the Western Fuel Company here in San Francisco. I think I wrote Professor Parr relative to my chemical results on the mine samples, and he sent me a tabulation of his chemical results on the mine samples. The figures that I supplied in this tabulation were the moisture figures. I have not read over the testimony of Professor Parr which he gave upon the trial of this case. I had not finished my determinations at the time I wrote Professor a letter. I simply had given him

(Testimony of E. E. Somermeier.)

the determinations that I had completed. The figures I sent him were the data on the moisture determinations. The complete report contains determinations of heating and sulphate oxidation. The figures that I supplied Professor Parr were the moisture figures, and I received a copy of his moisture figures also. On the bottom of the page there was no other data, but there was a continuation of the discussion.

I was present at the time when Professor Parr took samples from the car at Nanaimo. We sampled the same part of the car under the shed, but the samples taken out in the yard were from separate cars. I took three samples and Professor Parr two. On the cars out in the yard, Professor Parr sampled one and I sampled two, but neither of the cars which I sampled was the car that Professor Parr sampled. I took my samples with me back to the laboratory. I have given a report of my analyses thereon. The sample that I took from the mine to the laboratory weighed 7 to 9 pounds.

Q. Did you reduce the weight of that for the purpose of your analysis?

A. We used that sample to get the final sample for analysis.

Q. What was the last sample that you made—what was the weight of the last sample which you analyzed?

A. That was pulverized and analyzed?

Q. Yes. A. About an ounce.

Q. Now, then, really the amount of coal which you

(Testimony of E. E. Somermeier.)

[1582—1519] analyzed was just one ounce in weight; is that so? A. It was less than that.

Q. Less than an ounce?

A. These chemical analyses are made on the basis of one gram samples, and one gram is one-twenty-eighth of an ounce.

Q. You took from the mine, from a certain place in the mine, a certain quantity of coal.

A. About 40 pounds came down from the seam.

Q. You reduced that down to what weight?

A. Before I took it out of the mine?

Q. Yes. A. To fill that can I took 7 or 8 lbs.

Q. Was that the can you took back with you to your laboratory? A. Yes.

Q. Then after bringing that can back to your laboratory you took an ounce of that can, did you?

A. No.

Q. How much did you analyze?

A. I analyzed less than an ounce, but I did not take just an ounce out of the can. I took the whole sample and reduced it down; by a method of quartering and subdividing we make the sample finer until the final sample, which is as fine as flour is, amounting to about one ounce.

Q. To about one ounce; what size would that be?

A. One ounce of coal in a finely pulverized condition—

Q. (Intg.) About the size of the tip of my thumb?

A. No, it will occupy the biggest part of a 4-ounce bottle, or a 2-ounce bottle I should say.

Q. That was pulverized you say, and from an

(Testimony of E. E. Somermeier.)

ounce [1583—1520] of that pulverized coal you determined the contents of the coal in that mine, did you—the moisture content of the coal in that mine?

A. Well, we determined a part of the moisture content from that pulverized sample. In order to answer that question fully I will have to go into the technical details of making a chemical analysis of coal.

Q. I don't think that is necessary. We understand that you analyzed ultimately about one ounce of pulverized coal for the purpose of determining the constituent elements in that coal? A. Yes.

Q. And in that particular sample?

A. But preliminary to the final pulverization I gave the sample an air-drying to allow it to come to an equilibrium with the air in the laboratory before I analyzed it, and that air-drying loss was figured into the final result, so as to get it back to the sample as taken at the mine.

Q. Did you submit to the air-drying process the 7 or 8 lbs., or just this one ounce that you pulverized?

A. The whole 7 or 8 pounds that I have just stated, I submitted the coarse sample to the air-drying process, so that when I crushed the sample to a powder it would not lose moisture; I took the utmost precaution to get the moisture content within that sample to the best of my ability.

Q. These tables which you have here indicate the analysis which was finally made of this one ounce

(Testimony of E. E. Somermeier.)

sample? A. Yes.

Q. And from your analysis of the one ounce sample you came to the determination that all of the coal [1584—1521] in that mine was of the same kind, did you?

A. Well, the analysis, with reference to the one ounce sample related to the air drying previous to that.

Q. But you came to the conclusion, did you not, that all of the coal in that mine was of the same character, and quality as the one-ounce sample which you finally determined after the coal had gone through the air-drying process?

A. No, I didn't come to the conclusion that all of the coal in that mine was exactly the same as that, but I came to the conclusion that that sample was representative of the coal in that mine.

Q. Representative of all the coal in that mine?

A. All the coal in that mine within a reasonable distance; I don't know how large that mine may be, but I should say if it is an ordinary mine, it was representative of that coal; but other samples taken in other parts of the mine might show a decided difference in ash and sulphur.

Q. Now, isn't it a fact—

Mr. OLNEY.—Q. (Intg.) How about the moisture content?

A. But the moisture does not vary appreciably in different seams of the mine.

Mr. SULLIVAN.—Q. You say the moisture content does not vary?

(Testimony of E. E. Somermeier.)

A. Appreciably; I did not say it did not vary.

(Examining counsel here referred to the report submitted by the witness on the samples he had secured at Nanaimo of Nanaimo coal.) [1585—1522]

Q. Where are the mine samples?

A. No. 3 and 4 in my table.

Q. Mine sample No. 3, hard coal, Nanaimo, 2.53.

A. Washington method.

Q. Washington method?

A. 2.97 Pittsburg method.

Q. 2.97 Pittsburg method?

A. No. 4, 2.61 Washington method, 2.93 Pittsburg method; that is the second mine sample.

Q. No. 4, 2.61, Washington method, 2.92, Pittsburg? A. Yes.

Q. Slack from top of car, Washington method, 1.74 Pittsburg method, 2.20?

A. Yes, that is the sample that I have explained was taken in the afternoon—

Q. Off the top of the car?

A. Off the top of the car when the humidity was .41.

Q. I see here on Professor Parr's table that he shows the moisture content on the north face slope about 3.55; that is considerably in excess of any analysis which you made?

A. Yes, that is ½ per cent; that is a small difference, when we are talking about the moisture in coal as a general proposition. I do not claim that the moisture in these samples in this mine do not vary within ½ a per cent.

(Testimony of E. E. Somermeier.)

Q. What is that?

A. I never made any statement that the moisture in this mine would not vary ½ a per cent.

Q. Well, now, I see here, Professor Parr says that the moisture or at least the moisture content of coal taken from the open car, No. 4—

A. The open car?

Q. (Continuing.) —Was 4.44 per cent. Now, what is your analysis? Did you take any coal from that same car?

A. I did not take any sample from that car. I took one from the car adjoining. **[1586—1523]**

Q. From the car adjoining?

A. Sample No. 2, on my table, 3.16%, Pittsburg method.

Q. 3.16%, Pittsburg method; sample No. 2, is that it? Which car is that?

A. Sample No. 2, car on track.

Q. According to the Washington method it is 2.71? A. Yes.

Q. Now, Professor Parr says here, I don't know whether it is the Washington or Pittsburg method.

A. The Pittsburg method is the method he used.

Q. He says here, 4.44?

A. Yes, that is the condition I expected when I saw the condition in which he took his sample, much wetter; Professor Parr's sample, quite a portion of it was appreciably wet to the eye, while the sample that I took upon the other car did not have nearly as much evidence of dampness.

Q. Had these two cars been on the track during

(Testimony of E. E. Somermeier.)

your entire stay there in Nanaimo?

A. They had been there according to statements.

Q. Never mind the statements; so far as you observed, when you got there you saw these cars adjoining one another? A. Yes.

Q. When you left Nanaimo these cars were adjoining one another? A. Yes.

Q. They were hooked on to one another?

A. Yes,

Q. They constituted parts of the same train, didn't they. A. Parts of the same train.

Q. And you took the top off of one car and Professor took the top off another car?

A. No; that was not my statement.

Q. What was the fact?

A. My statement was I **[1587—1524]** sampled that car.

Q. How did you sample it?

A. I got down from the top to the bottom in the car and took the sample off from various parts from top to bottom in order to get the moisture variation from the top to the bottom.

Q. What did Professor Parr do with his car, the same?

A. The same way; we had two Chinamen at work there for us an hour and a half getting that sample.

Q. You had two Chinamen doing it? A. Yes.

Q. Now, then, according to the Pittsburg method, you show the moisture content of 3.15 and Parr shows that the moisture content *are* 4.44?

(Testimony of E. E. Somermeier.)

A. That is over one per cent, isn't it? A. Yes.

Mr. OLNEY.—The witness has already expressly stated that they were not taken from the same car.

Mr. SULLIVAN.—I know they were not taken from the same car.

Q. That is 1.28 per cent difference between your analysis?

A. Yes, but they are on different cars.

Q. I understand they are different cars. If he had taken a sample from one end of the car and you had taken a sample from the other end of the car, in all probability there would have been some difference, would there not?

A. Yes, most certainly have been some difference.

Q. And in all probability there would be the same difference.

A. No, I do not say it falls under the same difference. [1588—1525]

Q. If there be no difference, no substantial difference between the coals taken from the same part of the mine, how can there be any substantial difference in the coals taken from different parts of the same car?

A. The car had been standing exposed to the weather, so far as I know, for 6 or 8 weeks, the mine had been shut down for almost two months; I know there had been no coal taken out.

Q. You did not see it raining while you were there? A. No.

Q. How long were you there?

A. I have a weather report from the station, and

(Testimony of E. E. Somermeier.)

there was considerable rain before I was there; but there was no rain while I was there.

Q. What is the difference between the Washington method and this Pittsburg method?

A. Well, that is a difference in method used in the two laboratories. The Pittsburg Laboratory uses one method of sampling, which Professor Lord and I originated at St. Louis; the Washington method, they have so many samples to analyze there, that they use what might be called an abbreviated method, one that is a little bit quicker.

Q. They get quicker results, do they not?

A. Yes, they put the samples through in greater numbers. And that method as a rule runs 3 or 4 10ths of a per cent below the result by the Pittsburgh or the method I use in the second column; and that is in line with the **[1589—1526]** testimony that I gave earlier, that the results published in Bulletin 41 are lower than the rule, because they are analyzed by the Washington Laboratory.

Q. There is a difference of 45 hundredths of one per cent between your Pittsburg method and the Washington method. A. Yes.

Q. Is that usual?

A. It is usual; that is just what I have been saying.

Q. But there is a difference between you and Professor Parr, between your analyses according to the Pittsburg method—

A. (Intg.) You must bear in mind we have not duplicate samples.

(Testimony of E. E. Somermeier.)

Q. What is that?

A. We have not duplicate samples there. The only sample that might be at all called a duplicate sample was the sample taken under the shed, and Professor Parr took his sample the next day after I took mine, and the car had been dug down into, you see, for 24 hours before he took his. I don't know what Professor Parr's result is there, but I undoubtedly believe it is a little bit lower than mine. I took my samples when the car was opened up; Professor Parr was not there that day, and he waited until the next day, and we went down and took a new sample; but he got a sample that had been opened up to the air for 24 hours and the temperature in that room—it was a galvanized building, and was so high that undoubtedly he got a lower moisture than I did. If you will read it, Mr. Sullivan, I think it will verify that statement. **[1590—1527]**

I don't know how long the coal was in the car that I sampled, but I understood about six weeks. It rained during that period of time on the car outside. Said car was at all times exposed to the weather. The amount of oxidation for that time would be 2 or 3 tenths of a per cent. I took two samples from the body of the mine itself—samples 3 and 4. They contained 2.97 and 2.92 per cent. The sample that I got from the car had been exposed to the rain and the elements for six weeks and showed a moisture content of 3.16. I understand that the Nanaimo weather reports show that there had been ½ of an inch of rain upon the coal that

(Testimony of E. E. Somermeier.)
was exposed to the weather in the car. As to my opinion as an expert, as to how much that rain ought to have increased the weight of the coal, I should judge that from moisture there ought to have been an increase of 3 or 4 tenths of a per cent. From oxidation there should be an increase during those six weeks of 3 or 4 tenths of a per cent also. There would be an appreciable evaporation during those six weeks, however.

Q. Now, then, in addition to the rainfall there would be an increase resulting from oxidation?

A. There would be three factors there of an increase from rainfall, a slight increase from oxidation and I don't know what the loss would be from evaporation during six summer months, but it would be appreciable.

Q. Is the climate of Nanaimo similar to that in San Francisco?

A. The climate of Nanaimo is somewhat similar to San Francisco as far as I know. You must bear in mind that the rainfall on top of a car one day and the next day the sun comes out, the moisture that don't get into the interior will evaporate off again, and that coal will dry off on the surface.

Q. But your sample went down to the bottom of the car; you took the moisture of the coal, the moisture resulting from the rain?

A. I took what moisture was **[1591—1528]** there; I don't know where it came from.

Q. I will direct your attention to these figures here; you say that the sample taken from the mine

(Testimony of E. E. Somermeier.)

showed a moisture content according to the Pittsburg method of 2.97; the sample taken from the open car showed a moisture content of 3.16, after six weeks' exposure on the car, a difference of .19 of one per cent. Now, there is a difference of .19 of one per cent between the moisture content of the coal taken from the mine and the moisture content of the coal taken from the open car which had been exposed to the rain and the elements for six weeks, and upon which ½ an inch of rain fell; there is only a difference of .19 per cent. How do you explain that according to the theory you have announced here?

A. The sun was shining on the car most of the time during that six weeks.

Q. The same sun shines in San Francisco as shines up there, does it not? A. Certainly.

Q. And the moisture permeates through the mass of coal according to the duration of exposure to the rain, does it not?

A. It is according to the amount of rainfall, yes.

Q. How deep down in that car do you suppose during this period of six weeks, with one-half an inch of rain distributed over that period, do you think it sank into the mass of coal?

A. I have not the daily rainfall at hand, but as I have stated, unless the rainfall is sufficient at **[1592—1529]** any time to get down into the sample, the next day the sun will evaporate it off.

(By consent of counsel for the prosecution and by

(Testimony of E. E. Somermeier.)

permission of the Court, the witness Somermeier was here withdrawn from the stand so that President J. C. Branner, of Stanford University, might be at this time examined as a witness.)

[Testimony of J. C. Branner, for Defendants.]

J. C. BRANNER, a witness called for the defendants and sworn, testified as follows:

Direct Examination by Mr. OLNEY.

I reside now and have resided for 23 years at Stanford University. I am President of the University and Professor of Geology. I am by profession a geologist and have followed that profession since 1874. I have had experience with coal and have made a study of the subject of coal. I was Assistant Geologist on the Brazilian Geological Survey in South America for a number of years. For two years I was on the Anthracite Coal Survey of Pennsylvania and for seven or eight years I was State Geologist of Arkansas, having full charge of the work in the coal regions there.

Q. Doctor, does coal vary in weight at different times and under different conditions, that is, take the same lot of coal, will it vary from time to time in weight depending upon the condition under which it finds itself, or to which it is exposed?

A. Yes, sir, it varies somewhat according to **[1593—1530]** circumstances. Of course, as everyone knows, when coal is taken out of the mine, if it is wet, it weighs a certain amount, and that same coal, if it dries out, will lose moisture and become

(Testimony of J. C. Branner.)

lighter, or if you get moisture on it, it will be heavier; and it will vary in that respect, according to the condition of the coal, itself, that is, whether it is fine coal or coarse coal.

Q. That is, there is a difference between the extent to which this coal will vary in weight, because of addition or loss of moisture and the extent to which lump coal may vary in weight for the same reason?

A. Yes, sir; the lump coal does not vary so much in the moisture content of large bodies of it, as the fine coal; but if you take coal, for instance, in the anthracite regions of Pennsylvania, the coal is there crushed up, as everyone knows, and run through screens, and these screens separate out the coal into different sizes, and the screenings are usually thrown away there and heaped up in great piles called culm heaps, and those screenings contain a great deal more moisture than the coal of regular sizes, and larger lumps, and the finer those screenings are the more moisture they will hold. That is a well known fact, however, a fact everybody is acquainted with there.

Q. Would the same thing be true of bituminous coal, that is, as between screenings and lump coal?

A. Quite so, in fact, the bituminous coal is more friable than the anthracite coal is, and consequently what are known as the fines are usually broken up into finer fragments than those we have in the anthracite regions. **[1594—1531]**

Q. Is this difference in weight experienced in coal, or found in coal when it is handled in commercial quantities and in a commercial way, and in the course

(Testimony of J. C. Branner.)

of that handling is exposed to the weather?

A. Well, that is generally accepted in the coal business where I have had to do with it, in the matter of observation, it is accepted as a well-known fact that when the coal is wet it weighs more than when it is dry.

Q. Do coals from different mines, or coals from a different character vary as to what I may call the inherent moisture in them, that is, the amount of moisture that is found in them as they are in the mine?

A. Yes, they vary a great deal, that is, a great deal—I should say some of the coals, for instance, the Arkansas coals used to run less than one per cent of moisture, and from that they ran all the way up to over 40 per cent, although that 40 per cent was in what is commonly known as a lignite, but the bulk of the coals there in that state were what is known as low moisture coals, that is, coals that would contain from 4 per cent moisture down, and usually about 3 per cent.

Q. Doctor, take the case of a coal with a low moisture content, that is, an inherent moisture content of between 3 and 4 per cent, for instance, is there any rule as to how that coal will increase in weight, or decrease in weight, under normal weather conditions, if exposed to the weather?

A. Why, yes, the general rule is a matter of common information; **[1595—1532]** I presume it is known that if you leave the coal out in the wet, that is, coal that is the common run of coal that comes off the cars, screenings and all mixed up together, if you

(Testimony of J. C. Branner.)

let that coal get wet, why, of course, it will weigh a good deal more than it did before it was wet, when it was dry.

Q. After it is wet, Doctor, if you take the average run of coal, lump and screenings mixed, after it is wet, will it lose that moisture easily?

A. It depends a good deal upon how the coal is stored; for instance, we find in Pennsylvania, if you take the lump coal and store it by itself, in the early days when I went in the anthracite regions, they just got well into the business of breaking the coal and screening it. They found this, in regard to the storage of that coal, if you take coal and store it, what is known as egg coal, or nut coal, or lump coal, or any of the large sizes and store that in bins, where it is open to the weather, when the rain fell on that coal, it usually went through it very rapidly. But on screenings, the so-called culm heaps were a great bother, in a way, because in the winter time, they would be covered with snow, and rains were commonly heavy in the winter and in the early spring, and the moisture would fall on those great culm banks and saturate them with moisture, and little springs would actually come out around the bottom of them, there was so much of the moisture falling out of that coal; of course, in time, there was a layer over the surface that gave up its moisture and dried out, but the great mass of those culm heaps were found to be wet down to—well, I could not tell you what depth. **[1596—1533]**

Q. Take the case, Doctor, of a pile of coal contain-

(Testimony of J. C. Branner.)

ing, say, 24 per cent or more of fine material, being built up to an average height, say, of ten feet, and exposed during the winter; we start with a low moisture coal, that is, a coal with a low moisture content when the pile is made; now, suppose that pile is built, as I say, in the beginning of winter, and that it stands during the winter and the following summer, for a year; at the end of that year, could you say whether it would lose or increase in weight?

Mr. ROCHE.—Do you mean a winter in San Francisco, or a winter back east?

Mr. OLNEY.—Yes, a winter in San Francisco.

A. May I ask whether you mean that coal is open and exposed to the weather, or is it under cover?

Q. Oh, yes, it is exposed to the weather, always?

A. I should think if it was raining, the weather would soak down into the veins, and those veins would retain their moisture for a long, long while; I would not undertake to say how long, to be sure.

Q. Do you think they would retain it for a month, or two months?

A. Well, I should think they would, yes.

Q. Doctor Branner, take the case of a firm importing into this port coals of moisture content of from 3 to 4 per cent, and with a percentage of fines equal to 25 per cent or upwards, and importing this coal by vessel, and under deck and protected from the weather, so that when the coals were unloaded here their moisture content was low, that is, it was 3 or 4 per cent, and suppose the coals, on being unloaded here, are stored in the open and exposed to the

(Testimony of J. C. Branner.)

weather while waiting sale for shorter or longer periods, **[1597—1534]** as the case may be, and suppose the firm follows this course of business during a course of years here in San Francisco, is it possible to say either that on the average and over a course of years the coal would increase in weight, or decrease in weight, or is it impossible to say what the result would be?

* * * * * * * * *

A. I should think it would be perfectly reasonable to suppose that the coal would weigh more after it had been wet than before.

Mr. OLNEY.—Q. I am asking you, Doctor, if that would be true of a concern doing a commercial business in this manner, over a course of years?

A. Why, I should think so.

Cross-examination by Mr. SULLIVAN.

I should think that coal containing moisture varying from 1.70 to 4.40 per cent would be considered low moisture coal. I do not think it is true that low moisture coal will absorb less moisture in a given time under the same conditions than high moisture coal. I should think that the low moisture coal would absorb more. The low moisture quality or the high moisture quality of coal is not due to any extent to the porosity of the coal. I cannot testify precisely as to the point of saturation in coals. I have no definite figures as to the point at which Nanaimo coal will cease to take up any more moisture. I never have examined Nanaimo, or Wellington or British Columbia coal, but I am acquainted with it as I have

(Testimony of J. C. Branner.)

seen it in the market. **[1598—1535]**

Q. Assume that a load of that coal comes here to San Francisco, in dry weather, and is exposed for, say, ten, twenty, or thirty days, in the absence of any rain, and assume that the moisture percentage varies from 2.53 to 4.44, would not that coal lose weight by reason of the exposure to the atmosphere?

A. I don't think so.

Q. In dry weather?

A. I don't think so. Excuse me, I mean if that moisture content that you refer to there is the moisture content as stated in an analysis, that moisture would not be lost.

Q. That moisture, itself, would not be lost?

A No.

Q. But would the coal take on any additional moisture if it did not rain? A. I should not think so.

Q. You should not think so? A. No.

Q. You would think that in dry weather, in the absence of rain, the moisture content would not be altered at all? A. I should not expect it to.

Q. You should not expect it to? A. No, sir.

Q. Now, assume a case of this kind, Doctor, a cargo of 3,000 or 4,000 tons of this coal is brought from Nanaimo to San Francisco, in dry weather, in the summer-time, when the ordinary summer winds prevail, and there is no rainfall, and assume that when the coal comes here it is discharged into bunkers of the dealer in coal, and is during the prevalence of dry weather, within a very few days, transferred from the bunkers into barges, and within a few days,

(Testimony of J. C. Branner.)

say ten days, is transferred from barges into ships, for fuel purposes, no rain, mind you, in the meantime falling at all, in your opinion would that coal be affected in weight?

A. I should not think so. **[1599—1536]**

Q. In other words, you would think that the coal, when placed in the ship from the barge, would be equivalent in weight to the weight of the coal when transferred from the mine into the ship.

A. I should think so.

Q. To what extent, Doctor, do you suppose that cargo of coal, in rainy weather, would be affected by rain, assuming that we have, inside of 30 days a proportionate amount of rain that we have in the rainy season, and we have, I think, about 22½ inches in a year; and assume that the ordinary rainfall in the winter-time would occur, to what extent do you suppose the weight of that coal would be increased?

A. I would not undertake to say just how much.

Q. Well, it would be a to slight extent, do you think?

A. It would depend, you see, upon the amount of fines in that coal.

Q. Assume about 25 per cent of fines there. Now, assume these conditions, that is transferred from the mine to the ship at Nanaimo, the vessel after four days arrived in San Francisco, the coal is handled and transferred from the ship into bunkers, and within a few days after that is transferred from the bunkers onto a barge, and within ten days is transferred from the barge into the liner for fuel purposes,

(Testimony of J. C. Branner.)

all of the handling occurring, say, within the period of thirty days, in the winter-time, and assume, further, that there are about 25 per cent of fines in the coal; now, taking into consideration the character of the weather we have in San Francisco, the ordinary weather in the winter-time, where we have sunshine one day and rain the next day, would you say that the increase in weight would **[1600—1537]** exceed ½ of 1 per cent under those circumstances?

A. I should say it would be decidedly more than that.

Q. To what extent would you say, Doctor?

A. I would not like to say the percentage, because that is a matter for actual determination. If somebody told me that that increased two, three, or four per cent, I should consent to it, and would be interested to know what the facts were.

Q. And if somebody told you it did not increase any more than ½ of 1 per cent, you would not consider it impossible, would you, depending altogether upon the quantity of rain that fell on the coal?

A. It would depend on that, and the handling that the coal had, and the opportunities for water and evaporation, and so on, and the fineness of the screenings mixed up with the coal.

Q. And if, in the meantime, during the period of 30 days, there was, say, for half the time the sun shining brightly and no rain falling, would you not consider that the effect of the sun and the atmosphere on the coal would tend, in a great measure, to remove a great deal of the moisture?

(Testimony of J. C. Branner.)

A. It would remove the moisture decidedly, yes.

Q. The moisture in coal of that kind is merely a mechanical mixture, is it not, it is not a chemical mixture? A. Yes, it is mechanical.

Q. And if coal was allowed to rest in the pile for any length of time, by degrees the water will seep to the bottom, will it not?

A. Not altogether. The capillary attraction holds it among the fines, and it won't run out from the fines alone.

Q. But in a very short time it leaves the lumps, **[1601—1538]** except by being attached by reason of the fines surrounding the lumps, does it not?

A. Yes.

Q. Mr. Olney put to you a hypothetical question, assuming that there was a pile in the yard of the defendant, 10 feet high, containing about 25 per cent of fines, and the rain fell upon that pile, that is, the rain that we ordinarily have in San Francisco. Would not that coal lose this moisture within a period of 60 days, if the sun shone and we had no additional rain?

A. I would not like to express an opinion about a thing of that kind.

Q. Well, do you think, Doctor, it would lose this additional moisture say within a period of two or three months, if during those two or three months we had no rain at all?

A. I don't know even about that.

Q. But you are sure, are you not, Doctor, that it would lose a considerable amount of that moisture?

(Testimony of J. C. Branner.)

A. Oh, yes, the moisture evaporates away from the surface, and that evaporation continues down to a certain depth. And I have seen coal that is stored in some such fashion as that, but I would not undertake to say just the depth, and had been lying for months, and the fine coal in the bottom of it perfectly wet after it had been lying there for months.

Q. That would be away down at the bottom of the heap, that it was perfectly wet, would it not?

A. Towards the bottom, yes; just how thick the dried out portion over the surface is, I have not paid any particular attention to.

Q. You refer to a pile of screenings, don't you, in that answer of yours?

A. No; where you have coal unloaded from barges or coal cars, you know the tendency is for the lumps to all run away and to collect around the bottom, **[1602—1539]** and the screenings will keep heaping up and from the mass of the cone, so that when that begins to dry out, it will dry in a thin layer all around the surface, but the mass of that is protected somehow or other by the overlying screenings and will retain the moisture for a long period of time; at least, that is my observation of it.

Most of my observation of coal has been in eastern states where it rains and snows considerably at times, and where the snow would remain upon the coal for months at a time.

Q. Now, assume that a cargo of coal was placed upon a ship at Nanaimo and transported to San Francisco and the coal is four days in transit to this

(Testimony of J. C. Branner.)

port, and assume that the hatches of the vessel are closed in transit, in your opinion would there be any appreciable diminution of the weight of that coal during that period of time?

A. I should not think so.

Q. And, in your opinion, would there be any appreciable increase in the weight of that coal during that period of time? A. Not if it is kept dry.

Q. The oxidation during that period of time would be practically nil, would it not?

A. It would amount to but very little, indeed.

Q. It would be negligible, would it not?

A. I think so.

Q. Suppose that character of coal came here to San Francisco and was transferred from a ship to a bunker and from the bunker to a barge, and from the barge to another ship, for fuel purposes, in dry weather, the entire period of time from the date of shipment to the **[1603—1540]** date of discharge into the liner being about 30 days, the oxidation during that period of time would be practically nil, would it not? A. I should think so.

Q. It would amount to nothing; and assume, Doctor, that a pile of coal containing 32,000 tons were exposed to atmospheric influences for about 60 or 90 days, in dry weather in San Francisco, here, wouldn't the oxidation of that coal be practically nil?

A. I think that would be a negligible quantity.

Q. By that you mean practically nothing, do you not, Doctor? A. I do, yes.

Q. There would be practically no change in the

(Testimony of J. C. Branner.)

weight by reason of oxidation of the coal?

A. You appeal to me there as a geologist, and when we deal with periods there, there is some difficulty in understanding each other. I should say practically that the oxidation of coal, while we know that it goes on to a very considerable extent, it takes such long periods of time for it to oxidize, I should think that in commercial transactions, it is a negligible quantity.

Q. So that in commercial transactions, you would consider the oxidation to be nearly negligible. Now, assume, Doctor, that the coal is in storage 6 days, in dry weather, in your opinion the oxidation during that period of time would be practically nil, would it not? A. I think so.

Q. And wouldn't it be so little that it wouldn't be worth consideration at all, by any chemist or analyst —6 days?

A. Well, there are certain changes going about in coal, sometimes, where it has a good deal of iron pyrites in it,—that some people might insist upon as having more or **[1604—1541]** less importance, but I have never been disposed to regard it as of any particular importance.

Q. Assume, Doctor, that in this coal, there, the sulphur in the coal varies in extent from a fraction of 1 per cent to 1.27 per cent, would not that amount of sulphur in the coal be of such a slight quantity that the oxidation would be practically nil for any period of time?

A. Well, I should not say for any period of time.

(Testimony of J. C. Branner.)

Q. Well, say for that month?

A. I don't think it would be of any particular importance.

Q. Or for 6 months; don't you think the oxidation for a period of 6 months would be practically nil?

A. It would be very little.

Q. And in 2 months you think that the amount of oxidation would not be apparent, don't you?

A. Well, it might be apparent.

Q. I mean in so far as increase of weight is concerned.

A. Commercially, I should not think it would be important.

Q. Is it not true that the cause of oxidation generally is the amount of sulphur in combination, or free in the coal? What causes oxidation, in your opinion, Doctor?

A. It is absorption of oxygen from the atmosphere, but that takes place under a great many different conditions. We have oxidation in coal, and oxidation in other kinds of minerals; it depends on what they are.

Q. Oxidation is nothing more or less than a very slow combustion of the coal, itself, by reason of the combination of **[1605—1542]** oxygen with the substance of the coal, or with the sulphur of the coal?

A. The atmosphere, yes.

Q. Is the oxidation caused by free oxygen, or is it caused by the oxygen contained in chemical combination with water, or both?

A. Well, I don't know that I can explain exactly

(Testimony of J. C. Branner.)

the processes of oxidation. We generally speak of it in general terms as decomposition, when we speak of it in connection with coal that is exposed naturally. For instance, coal that has been exposed in that way, it is spoken of in Pennsylvania—let me see what they call it—coal blossom; it is simply a black, sooty coal where the coal has been entirely decomposed, disintegrated.

Q. Does it make any difference in the degree of oxidation in the coal, say coal of the quality of this Wellington coal, whether it is exposed to water, or not, to rain, say, within a period of say 30 or 60 days?

A. I don't know how far the water from the rain has influence on that sort of thing, but I remember we used to be troubled sometimes in Pennsylvania, and in other parts of the country where I have been acquainted with coal mining, with spontaneous combustion in the coal, the coal would catch fire, and usually it has been attributed to the presence of sulphur in the iron pyrites, that is very often found in the slates that accompany the coal.

Q. When coal is analyzed and the analyst finds a certain amount of sulphur, say 1.9 per cent of sulphur in the coal, by that he means to say that he discovers that sulphur in combination with iron, or some other mineral?

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A. That is true. **[1606—1543]**

Mr. SULLIVAN.—Q. Now, assume, Doctor, that the pile of coal referred to in Mr. Olney's question was not constant, but was being moved, changed

(Testimony of J. C. Branner.)

every thirty or sixty days, wouldn't the amount of oxidation in that pile of coal be also nil?

A. I think so.

Q. And would not the amount of loss, or the amount of increase, would not the amount of increase in weight in dry weather be practically nil?

A. Increase in dry weather—

Q. Yes, the increase in weight by reason of moisture, in dry weather. A. Yes.

Q. Is it not a fact, also, Doctor, that if this pile of coal were kept moving completely every thirty or sixty days, in ordinary winter weather, like we have here in San Francisco, would not the increase by the oxidation in that pile of coal be practically nil?

A. I think so.

Q. And don't you think, also, Doctor, that the increase in weight by reason of moisture in this pile would be comparatively slight, where it is kept moving and completely moved in 30 or 60 days?

A. Of course, if the coal were handled, shoveled, or anything of that kind, so that the atmosphere could get at it, the moisture would soon disappear out of it.

Q. If this pile of coal is sold to the wholesale dealers, retail dealers, sold to the trade, sold to ships, and is transferred from place to place by means of shovels into buckets, and by means of buckets into bunkers, and by means of chutes from the bunkers into the barges, and by means of buckets from the barges into ships, **[1607—1544]** and by means of shovels from yards into wagons, and so forth, continual motion affecting this pile of coal to the extent that the origi-

(Testimony of J. C. Branner.)

nal pile disappears say in 30 or 60 days at most, now, under those circumstances, would not the increase of weight or moisture in the ordinary winter weather in San Francisco be very slight indeed?

A. Well, I am sure I would not know exactly how to answer that question, any farther than to say that, of course, the stirring and handling of coal gives the moisture an opportunity to escape.

Q. And, in your opinion, it would escape; is that not so?

A. At every opportunity it had, it certainly would.

Redirect Examination by Mr. OLNEY.

Mr. OLNEY.—Q. Doctor, when you say the moisture would escape, you mean that some of it would escape? A. I do; yes.

Q. Coming to the very question that was asked of you by Mr. Sullivan, speaking of a cargo coming in here in wet weather, composed of 25 per cent or upwards of fines, and being rained on after it was delivered here, and presumably while it was in the storage bin, in the bunker, which is exposed to the air, exposed to the heavens, or in a pile and getting wet in that way, during the period of 15 days—half a month—and then remaining there exposed to the sun for 15 days longer, now, in response to a question by Mr. Sullivan you said that under those circumstances it would lose moisture; to what extent would a pile of [1608—1545] coal made up of fines in that way, and wet, lose moisture in the course of 15 days, even though exposed to a hot sun?

Mr. ROCHE.—Made up of fines?

(Testimony of J. C. Branner.)

Mr. OLNEY.—(Continuing.) Made up of fines to the extent of 25 per cent; and take the case, Doctor, of a pile of coal made as they usually make piles, by taking it from a height, so that the lump coal spreads around the edges of the pile and the fine coal is in the middle and runs up, take a pile of that sort, such as you find in ordinary commercial uses, 25 per cent of fines, and the balance wet, and suppose it is wet for half a month, and then it is dry for 15 days and it is exposed to the sun, the hot sun, is it going to lose any substantial portion of its moisture in those 15 days?

A. I should not be able to answer that question in any way that would be satisfactory to myself. From my observation of the packing of coal, you can get an idea of some of the things that happen in a coal heap, if you imagine a couple of pieces of glass with a drop of water put in between them and press those two pieces of glass right down together; now, the atmosphere cannot get at that glass except through the margins, right in between, and the crack in there is very small; if that drop of water has a little mineral matter in it, it very frequently happens that the drying out of that water around the margins there, where the air cannot get it, will practically seal—shut out the atmosphere, so that the moisture cannot entirely disappear at all, so that you can leave those two pieces of glass there for days at a time, or months, and still that water will never entirely evaporate; now, the same thing happens between two small fragments of coal, or anything of that kind, and that you have packed down together; you take those two [1609—

(Testimony of J. C. Branner.)

1546] faces that are smooth and press them up against each other, with water in between, and where the water begins to dry out, wherever the air can get at it through loose material, the water will disappear; how deep, though, depends on circumstances, the blowing of wind makes a good deal of difference; those two pieces will stick together and keep that moisture in there almost indefinitely, under favorable circumstances, of course, you understand; but to say how long it would take water to dry out of a pile of fine coal, I would not want to say, I don't know.

Q. I don't want to ask you, Doctor, how long it would take to dry it out, but I was wondering if you could give us an idea if it would dry out substantially through or only in the lower levels, leaving the lower levels wet, say in 15 days. Take a pile of coal say 10 feet high on an average, and it is wet, thoroughly wet, when it comes in there, it has a proportion of fines in it of 2 per cent, and the fines are in the center, as it were, with the lump coal around the edges; take that pile 10 feet high and expose it for 15 days to a real hot sun, say a sun such as we sometimes have here, with our north winds, would you think, for instance, that coal would dry out to a depth of more than a foot from the surface in those 15 days? **[1610—1547]**

A. Well, I should say it would depend entirely on the character of the top layers of that coal. If you had fine coal at the surface, then it would dry out more slowly. That is about all I can say. If it is larger lumps, it would dry out more rapidly.

Mr. OLNEY.—Q. And you would not want to say,

(Testimony of J. C. Branner.)

taking it, for instance, where the top of the pile is very largely made up of fines, you would not want to say that even in that case it would dry out in the period of 15 days only to a foot, or more or less, you would not have any idea of that?

A. I would not want to say.

Q. Doctor, is the process of oxidation always a process by which the weight is increased, provided it does not reach the point of ignition; take it at normal temperatures?

A. No, oxidation does not always increase the weight of coal.

Q. I should have confined my question, really, to the oxidation of coal, and oxidation particularly of the sulphur in the coal; is that process one that involves an addition in weight when going on at normal temperatures?

A. Of course it depends upon what minerals are formed. You take some minerals that are formed that way, they increase in weight, they take up water and hold onto that water.

Q. I am speaking of coal, Doctor, the oxidation of coal.

A. When the oxidation of coal takes place, sometimes these minerals are formed in the process. **[1611—1548]**

Q. What I wanted to get at was, if oxidation is going on, if the process is going on, is there an increase in weight going on at the same time; in other words, does that process, so far as the oxidation goes on, involve an increase of weight?

(Testimony of J. C. Branner.)

A. Yes, in the oxidation of coal, it increases the weight of it. The point that I want to make, and that I hope to express in connection with the subject of oxidation is, that although that process is a process in common oxidation as it takes place in the open. it is so low that in a case of this kind it is a thing that it seems to me might be neglected.

Q. It is a process, however, is it not, Doctor, that the rapidity of it increases decidedly with an increase in temperature? A. Yes.

Recross-examination by Mr. SULLIVAN.

I was, as I have said, at the head of the Geological Department of Arkansas for seven or eight years. I had charge in that capacity of the whole coal-field in the state, and became familiar with storages of coal deposits—30,000 or 40,000 tons in a pile. As State Geologist, I had occasion to inquire into the conditions that caused deterioration of coal and its heating powers, etc. In Pennsylvania, as assistant in the Pennsylvania Survey, we were studying the anthracite. I have observed the changes in coal stored for periods, say, of a year.

Q. You never saw any changes in the weight or increase in the weight of coal by reason of oxidation **[1612—1549]** which went on for a period of a year, in a pile of coal, did you, Doctor?

A. Yes, sir, I have seen some oxidation go on that way, but I never paid any particular attention to it, because I was never impressed by it, I never considered it of importance.

Q. Didn't you always, in dealing with the question

(Testimony of J. C. Branner.)

of the change in the condition of coal, consider the effect of oxidation, as affecting the weight of the coal, as practically of no importance?

A. That is the way I looked at it.

Q. As always being a mere negligible quantity to be considered? A. Yes.

Redirect Examination by Mr. OLNEY.

Mr. OLNEY.—Q. Doctor, has this subject of the oxidation of coal been investigated particularly in the last few years, for instance, has the Bureau of Mines of the United States made a particular study into this subject?

A. I don't recall any particular study of that subject. I think that Professor Rogers, down at Stanford University, made some experiments on oxidation of certain forms of pyrites, and that might help out in a question of this kind. * * * Whether the United States Survey, or the Bureau of Mines, have carried on any investigation of that kind, also I could not tell you. They have published very extensive series of coal analyses in one of the bulletins of the Bureau of Mines, which, perhaps, contains the largest number of coal analyses to be found anywhere in the country, but I do not think they went into that phase of the question. **[1613—1550]**

[Testimony of E. E. Somermeier, for Defendants (Recalled—Cross-examination).]

E. E. SOMERMEIER, on the resumption of his cross-examination by Mr. Sullivan, testified as follows:

Sample Number 1, which was taken from the car-

(Testimony of E. E. Somermeier.)

load of coal in the shed, had been in that enclosed shed about six weeks, and was, of course, free from rainfall. The shed had plenty of ventilation.

(It was here explained that Sample Number 1 was in the shed, Sample Number 2 in the open, and that Numbers 3 and 4 came from the mine.)

I should say that the usual increase by oxidation in the weight of the Nanaimo coal, situated as was that coal for that period of time, would be 3/10 to 5/10 per cent. There would not be any absorption of moisture from the surrounding atmosphere where the coal came directly from the mine as when the humidity was almost up to the point of saturation. Sample Number 1 showed a moisture content of 2.75% according to the Pittsburgh method, and 2.33% according to the Washington method. There was a decrease in the moisture content of that coal as compared with the mine sample, the decrease being approximately 2/10 of a per cent. There was also a decrease in weight during those six weeks so far as the moisture is concerned. In answer to the question whether I made any analyses for the purpose of showing the net increase or net decrease in weight of that sample, I would say that it would be impossible to make that determination. In order to do so, I would have to be there at the time the car was put in and then at the end, too, and weigh the car at the beginning and at the end. In answer to the question whether I cannot make an analysis for the purpose of determining the increase or decrease resulting from the oxidation. I would reply

(Testimony of E. E. Somermeier.)

that I could make that analysis, but I could not tell what relation that would bear to the coal back [1614—1551] in the mine. Sample Number 3, namely, the mine sample, showed, according to the Washington method, a moisture content of 2.53%, and according to the Pittsburgh method, of 2.97%, the difference being 4/10 of a per cent. Sample Number 4 showed a moisture content by the Pittsburgh method of 2.92, and by the Washington method of 2.61. If we are going to compare samples against samples, however, we should take the same method. Sample Number 5 was the slack taken from the top of a car on the track exposed to the weather, presumably for about 6 weeks. It had rained during those 6 weeks. The moisture content, of that coal, according to the Pittsburgh method, was 2.20, and according to the Washington method, 1.74.

Q. Now, then, according to that analysis, after having been exposed in the open air for six weeks, and having suffered from a rainfall of ½ an inch, the moisture content of that sample was 0.77 of one per cent, was it not? A. Yes, sir.

Q. That is, less; that is, notwithstanding the fact that this slack coal taken from the top of a car, having been exposed to the elements for six weeks, and ½ an inch of rain there was less moisture in the coal that you analyzed taken as you say than there was in the coal as taken from the mine?

A. Yes; I knew that before I analyzed it.

(Testimony of E. E. Somermeier.)

Q. A difference of 77 one-hundredths of one per cent? A. Yes.

Q. Wasn't there also a corresponding decrease in the weight of that coal?

A. That particular skim on top, **[1615—1552]** certainly, but that is what I took that sample for, to see what the extreme air drying was; the bulk of the car underneath was entirely a different proposition and would be presumably something like my sample No. 2.

Sample Number 1 was lump coal, showing 2.33% moisture content, according to Washington method and 2.75½ according to Pittsburgh method.

Q. And it weighed less than—that lump coal contained less moisture than the sample taken from the mine itself. A. Yes, that's all right.

Q. How much less?

A. About 0.25%, between 0.25 and 0.2.

Q. Did not that coal also, notwithstanding the exposure to the elements for six weeks, weigh less than the sample taken from the mine?

A. It weighed less, as far as that moisture goes, but as I told you before, there was probably 3-tenths or 4-tenths or 5-tenths of one per cent exposed, which would increase it, so if you want my opinion on what the weight of that car was at the time I had taken the sample compared with what it was at the time it came out of the mine, I would say it was perhaps 0.25 increase.

I did not make any direct determinations of the increase or decrease in the weight of coal resulting

(Testimony of E. E. Somermeier.)

from oxidation. That was left to Professor Folsom. [1616—1553]

Q. Now, not having made any examination or investigation for the purpose of determining the increase in weight resulting from oxidation, how are you able to make an estimate now or give your opinion as to the increase in weight of all of these coals possessed by the Western Fuel Company resulting from oxidation?

A. Mr. Sullivan, I have analyzed and tested coals from all over the United States, and I don't imagine that this coal up in British Columbia is one coal out of a thousand; and they all show increases that have ever been tested.

*　*　*　*　*　*　*　*　*

Q. Did you not know, at the time you were employed by the defense in this case, that it was important for the defense to establish the increase in weight, if the increase occurred, in the coal that was imported into San Francisco and transferred from the barges of the Western Fuel Company into the ships of the Pacific Mail Steamship Company?

A. I was familiar with the points in controversy, yes.

Q. And didn't you understand that that was one of the principal points in controversy?

A. In increase in weight?

Q. To establish a legitimate increase in weight in the coal that was sold by the Western Fuel Company to the Pacific Mail Steamship Company?

A. No, sir; I was under the understanding it was

(Testimony of E. E. Somermeier.)

as to whether that increase could be accounted for by natural causes; legitimate did not have anything to do with it. **[1617—1554]**

Q. You *include among* the natural causes of oxidation, do you not? A. Yes, sir.

Q. Why, then, if you understood that that was one of the questions involved in this defense here why, then, did you not make an analysis or an investigation for the purpose of determining the increase in weight, if any given quantity of this particular coal resulting from oxidation within a given period of time?

A. Because I was under the impression that this trial was set for October, and I didn't have sufficient time at my command to make an exhaustive test.

Q. You couldn't make a test between August and October.

A. I didn't get back to Columbus until the 13th of September.

Q. And you understood the case was set for trial in October? A. Yes.

Q. Don't you know that you have testified here to a certain percentage of increase in weight by oxidation within the period of a few days?

A. Well, sir, I have told you also that that oxidation experiment was left to Professor Folsom.

Q. Don't you know, according to your theory, that you can tell the extent of oxidation and the increase in the weight of coal resulting from oxidation within the period of six days or ten days after the coal is taken from the mine?

(Testimony of E. E. Somermeier.)

A. Why, you can get a figure by that time, certainly.

Q. Why. then did you not, for the purpose of [1618—1555] fortifying your testimony here, get a few sacks of coal and expose it to the elements for 10 days or 20 days or 30 days and come into court and testify here to the result of your examination after the expiration of 30 days for the purpose of showing that the quantity of coal increased in weight by reason of oxidation?

A. As I say, that test was turned over to Professor Folsom because he was here on the grounds. I am giving my opinion based on tests that cover a number of years and a great number of coals.

Q. How long have you been in San Francisco during this last visit here?

A. About two months, I think.

Q. About two months? A. It seems that long.

Q. Why didn't you, two months ago, take a ton of coal, more or less, and store it away somewhere, determine its weight to a nicety when it was stored away and then after the expiration of a month ascertain its weight for the purpose of explaining to this jury the increase in weight of that particular ton of coal resulting from oxidation?

A. Mr. Sullivan, such a test as that requires a well-equipped laboratory, and I was under the impression that I would probably be called here to testify in the course of 10 days.

Q. Do you remember seeing Mr. Tidwell and myself down at the bunkers one day when you were

(Testimony of E. E. Somermeier.)

there with Professor Parr?

A. I remember seeing some gentlemen down there; I didn't recognize you at the time, but I was told afterward it was Mr. Sullivan and Mr. Tidwell.

Q. That was about 2½ months ago, was it not?

A. Yes, sir.

Q. Why, at that time, when you were making an **[1619—1556]** investigation, and you noticed counsel for the Government making an investigation, why didn't you direct a certain amount of coal to be set aside and say, "Now, we will catch those fellows; we will show them that 10 tons or 100 tons of coal will increase in weight by reason of oxidation during 30 or 60 days"?

A. Because, as I told you, I expected I was going to be called every day for the last weeks, and there has been a series of oxidation tests carried on down at Stanford University and I would like to refer to the tables, if they were in evidence at this time.

Mr. OLNEY.—We have done that very thing, Mr. Sullivan, and we will furnish you with that information.

(Witness Continuing.) I suppose I could have had access to the laboratories of Stanford University and of the University of California.

I made a preliminary report here in San Francisco to counsel for the defendants, but that did not include any results on these samples of coal from Nanaimo and San Francisco because I had not analyzed them.

(Testimony of E. E. Somermeier.)

(Said preliminary report or letter was here handed by counsel for the defendants to Mr. Sullivan.)

This letter was written by me and this is my signature. A copy of the preliminary report of September 5th, referred to therein, is in the hands of counsel for the defendants.

(Said copy of said preliminary report of September 5th was here handed by counsel for the defendants to Mr. Sullivan.)

The signature attached to this report is my signature.

(Said report of September 5th was here introduced in evidence by the United States and reads as follows:) **[1620—1557]**

[U. S. Exhibit No. 163—Letter Dated San Francisco, September 5, 1913, E. E. Somermeier to McCutchen, Olney & Willard.]

"San Francisco, September 5, 1913.

McCutchen, Olney & Willard,

Dear Sirs: I submit herewith preliminary report on work and conditions at Nanaimo, B. C., and here at San Francisco. As you are aware, a strike is on at Nanaimo and as a result I was not able to secure any samples that would represent cargo shipments. However, both Professor Parr and myself secured two mine samples, one from each of the seams worked at Nanaimo, and also several samples from coal stored in cars in the yards.

"From data at hand and from the general appearance of the coal, it probably does not run as mined

(Testimony of E. E. Somermeier.)

over three per cent moisture. The coal as it comes from the mine is screened over a two and one-half inch screen and the over size run into five-ton cars. These cars, as a rule, are weighed the same day as mined and are then unloaded into a ship within three hours from the time of weighing. However, occasionally they stand over night, and possibly for a day or two, before being sent to the ship, and, according to Mr. Counely, the wharfmaster, the time between weighings and the time of being unloaded will average about five hours. The moisture in the coal in the mine is in equilibrium with air, having a humidity of about ninety per cent or over. During the greater part of the year, the dry season, as soon as mined the coal comes into air having a humidity of thirty-five to eighty per cent, and as a result it tends to lose moisture, the loss probably on an average being less than two, to three-tenths of one per cent. During loading, in dry weather, a small garden hose is occasionally used to wet down the coal during a portion of the time of loading, and, according to Mr. Counely, **[1621—1558]** it is used perhaps as much as ten hours in loading a five thousand ton cargo. The amount of moisture added to this way will probably not exceed five one-hundredths of one per cent. The net results of these gains and losses is that the coal loaded into the ship contains, perhaps, two-tenths per cent less moisture than the coal as mined.

"On voyage to San Francisco during the dry months there is probably a small additional loss, ap-

(Testimony of E. E. Somermeier.)

parently, however, not a very considerable one.

"At Northfield, on Departure Bay, the coal is protected from rainfall at all times so that conditions for moisture losses are somewhat more favorable at this point of shipment than at Nanaimo.

"In going over the yards at Nanaimo the method of shipment of the coal and method of weighing were noted particularly. The coal is weighed on a scale having a capacity of one hundred tons, the amount of coal weighed at one time being approximately five tons. Weighings are made very rapidly and only to the nearest hundred-weight. In talking with the men who did the weighing, they claimed to weigh with the beam at the center. However, with the rapid weighing that is necessary, it seems probable to me that their weight to the nearest hundred-weight would practically always be somewhat heavy and that in practice they would weigh to a falling beam. An error of one hundred pounds on five tons weighed, amounts to one per cent, and, while the errors may not all be in one direction, I think that they are cumulative toward the side of weighing too heavy. My opinions on the weighing, however, are in no sense to be taken as expert opinion, but general observations based on some familiarity with weighings as they are conducted in laboratory practice. **[1622—1559]**

"In regard to the work at San Francisco, in so far as sampling coal is concerned the situation is worse than at Nanaimo. No Nanaimo coal is available and the only samples that Professor Parr and myself

(Testimony of E. E. Somermeier.)

secured were samples of Australian and Japanese coal, taken from the bunkers at the Folsom Street dock. These samples cannot, of course, compare in importance to samples taken from cargo shipments of Nanaimo coal if such were obtainable. However, the results that may be obtained from them will serve as a supplement to the work carried on by Professor Folsom.

"On the assumption that the coal from Nanaimo usually arrives in San Francisco with a moisture content of approximately two and one-half per cent, the probable moisture losses during the summer months are not over 25/100 of one per cent, as the drying out is confined largely to the surface of the piles of stored coal. This coal, if thoroughly air dried, probably will not lose over one per cent of moisture, so that the assumption of 25/100 of one per cent as the loss for the entire pile is probably too high. During the wet months the moisture gained due to rainfall might run up to five or six per cent, or more. On the assumption that the coal is held in storage for an average period of three to four months, the amount of rainfall at San Francisco, twenty-two inches, is sufficient to increase the moisture in the coal three or four per cent. However, this assumption of an average storage period of three to four months means that fifty to seventy-five thousand tons of coal are kept on hand. The bunkers which I visited held less than ten thousand tons, so that if calculations as to effect of rainfall on

(Testimony of E. E. Somermeier.)

the coal, based on a storage period of three to **[1623 —1560]** four months, are to be used, the facts should be verified as to whether there is storage capacity for fifty to seventy-five thousand tons of coal and as to whether such has actually been on hand.

"In regard to the Government analyses on samples of coal sold to it, in my opinion the results from moisture are of little value to you in so far as establishing the moisture content of the coal at that time, as these results are certainly too low. This fact is admitted in Government Bulletin 63, page 8.

"Professor Parr and myself visited the Presidio and talked with the Assistant Quartermaster, who had charge of coal deliveries there. Their method of sampling will, undoubtedly, give low results from moisture. Furthermore, these samples are all analyzed at Washington, and the results for moisture upon such samples obtained at Washington, are lower than results obtained at Pittsburg, where the mine samples were analyzed. The difference in results between the two laboratories is about four-tenths per cent, as shown by comparison of results given in Bulletin 63, page 27.

"In regard to the increase in weight due to the oxidation of coal and to the formation of sulphate, these values can be approximated from data on other coals, but we have no data on the particular coals in question. The oxidation of coal in the time held in storage here is probably less than three-tenths of a per cent, which would correspond to less than three-tenths per cent increase in weight due to this cause.

(Testimony of E. E. Somermeier.)
The oxidation of sulphur to sulphate during storage here is probably between five-hundredths and two-tenths of a per cent, which would correspond to an increase in weight of the coal from three-tenths to one and two-tenths per cent. **[1624—1561]** The loss in weight due to escape of volatile gases while the coal is in storage is very probably less than five-hundredths of one per cent, or a result practically negligible.

"With this low moisture coal, and with the humidity and rainfall of San Francisco, the changes in weight are practically all increases. The increase due to rainfall may well be as high as three per cent, that due to oxidation of the coal itself up to three-tenths per cent, and that due to oxidation of sulphur to sulphate from three-tenths to one and two-tenths per cent, and that the total result is over three per cent is not at all surprising.

"The Government tests on carload lots of low moisture coals in many instances show increases in moisture of the car sample over the moisture in the mine sample of from one to three per cent, as shown by the following results from Bulletin 362 and Professional paper 48 of the S. S. G. S.

(Testimony of E. E. Somermeier.)

No. Sample.		Moisture in Mine Samples.	Moisture in Car Samples.
Jamestown	9	2.40–2.40	3.00–3.90
"	11	1.80–1.80	1.50–3.60
"	13	2.80–2.10	2.80–3.10 4.00
Alabama	1	1.22–1.35	2.34
"	2	2.25–2.42	3.36
Arkansas	1	1.02–0.75	3.24
"	2	0.95–0.78	2.23
"	3	1.60–1.63	2.19
"	5	1.38–1.80	2.36
Indian Ty.	2	1.46–1.30	4.45
"	3	2.97–2.93	4.61
[1625—1562]			
Kansas	1	2.91–3.50	4.99
"	2	2.44–2.36	4.18
"	3	2.01–2.54	2.50
Kentucky	1	2.91–2.85	3.10
"	4	4.61–4.76	5.27
Missouri	1	4.80–4.92	8.33
West Va.	1	1.40–1.35	1.75
"	7	2.48–2.12	3.94
"	8	1.90–1.84	4.16
"	9	1.98–1.77	4.08
"	11	2.21–3.05	4.07

"These car shipments were shipments to the World's Fair at St. Louis and to the Jamestown Exposition and were during the late summer and fall, or during the dry season of the year.

"As to the capacity of coal to hold moisture mechanically, in Professional Paper 48, page 253, the

(Testimony of E. E. Somermeier.)

results upon West Virginia No. 5 coal, which coal is fairly comparable to the coal brought in from Nanaimo, show moisture in the mine samples three per cent on washed coal at 1¼" size 4.84 per cent on fine slack 19.5 per cent. Other results on washed coal given in Professional Paper 48, show similar increases. The assumption of a possible increase of weight of three-tenths per cent, due to oxidation of the coal, is, in my opinion, very conservative, as, while data on this particular question is not very complete, it is a well-known fact that many coals take up large amounts of oxygen. This has been investigated by White and Porter and Chamberlain and the results appear in Government publications. Some of the published results on Wyoming coals show an apparent absorption **[1626—1563]** and increase of weight up to twenty per cent. This increase, however, is a result of many years of weathering, and no such results are to be obtained or expected in short intervals of time. However, the results obtained by Porter in the laboratory indicate that increases of three or four-tenths per cent are not unusual.

"Upon the completion of the tests upon the samples taken here at San Francisco and at Nanaimo, and upon a further study of Professor Folsom's results, I will be able to make a more complete and specific report. In my final report I shall also refer you to the specific results and conclusions of Messrs. Porter, White, Chamberlain, and others, on the oxidation of the coal material itself and the oxidation of sulphur and sulphate. I submit this report merely

(Testimony of E. E. Somermeier.)

as a preliminary in order to give you an opportunity to familiarize yourselves with my general opinions and conclusions up to date.

"Very respectfully yours,

"E. E. SOMERMEIER."

(Said document was here marked U. S. Exhibit 163.)

I refer in that report to Government tests. It is not true that the Government always makes its tests at the point of delivery or consumption. It is the sampling that is made at the point of consumption. The samples are then sent back to the laboratory at Washington, D. C., in four-pound cans, sealed and air-tight. The Government has been buying coal in San Francisco for a long time. I do not know whether they have been buying Western Fuel Company. These samples were analyzed in the Government laboratory. The analysis is supposed to determine the amount of moisture content in the samples. **[1627—1564]**

I say "supposed" because my opinion is that the Government analyses are all too low. The Government I know buys its coal with reference to the amount of heat units that is in it. The question of moisture is very important by reason of that fact. I do know of Government reports made where there was an analysis for the purpose of determining the increase in the weight of coal resulting from oxidation. I have made some of those tests myself at St. Louis, Missouri. The specifications of the Government in their coal contracts do not call for the change

(Testimony of E. E. Somermeier.)

resulting from oxidation because that is not necessary, the sample being sent immediately to the Government laboratory and analyzed as quickly as possible. Oxidation affects the heat value of coal. There is not any oxidation after the sample is taken. The Government is not interested in the oxidation that takes place before it purchases the coal. The Government is not concerned with what oxidation may have occurred between the mine and the time of purchase by the Government. I know that the Government has had coal stored at Panama, and now that you recall that incident to my mind, I remember that Mr. Porter has made an extended series of experiments of Panama coals for the determination of the loss of heat value resulting from oxidation. Mr. Porter has been making experiments for several years to determine the deterioration of coal in storage. I do not know that none of the coal purchased by the Government within the last year has been analyzed with the purpose in view of determining the amount of oxidation that will take place in that coal in any given time. On the contrary, Mr. Porter has published several reports of his work in this connection. I refer you to Porter & Ovitz, Technical Paper No. 2, United States Bureau of Mines, page 10; also Porter, Chemical Engineer, July, 1913, page 10. I do not have the **[1628—1565]** former, but I have the latter article here and will give it to you now.

I went to the Presidio during the course of my investigation. I did not get a table there from the Presidio officials showing the results of tests of coal

(Testimony of E. E. Somermeier.)

purchased by the Government. All I went out for was to find out their methods of taking samples. I did not investigate the result of their analyses.

Q. Did you find out from the quartermaster that the result of the analysis of the Nanaimo coal showed that the moisture content was but a little bit more in some cases and a little bit less in other cases than it was at the mine?

A. I made no effort to get any information as to the moisture content. What I was after was the method of getting the sample, and I passed judgment on their method of getting the sample. In my opinion their method of getting the sample is not good.

Q. It is the method under which the United States Government purchases about $6,000,000 worth of coal a year, is it not?

A. It does not come up to their specifications laid down in the book, the quartermaster told me out there.

Q. But you know that it is the method under which all the coal of the United States Government is purchased for the army and navy, do you not?

A. Well, the method as he told me they took it at there did not come up to what they have in print; it was decidedly a poor method of taking a sample. **[1629—1566]**

Q. But that is the method in vogue throughout the United States, is it not?

A. Well, it simply means, Mr. Sullivan, that they don't always live up to it in getting samples.

(Testimony of E. E. Somermeier.)

Q. But is not their method out there the same method that prevails all over the United States?

A. I don't know; I hope not, because their method was not what it was supposed to be in the Bulletins.

Q. You know that that coal was received by the Government that comes from Nanaimo is transferred from ships to bunkers and from bunkers to barges and from the barges say to transports, where transports get coal? A. Yes.

Q. And do you know how the coal gets to the army station?

A. No—yes, I know how it goes to the Presidio; it goes in wagons.

Q. It goes in wagons to the Presidio? A. Yes.

Q. Don't you know, according to the method in vogue at the Presidio where you visited, that the analysis would show the moisture content at the mine and also the moisture content at the place of delivery?

A. It cannot show both at the same time, Mr. Sullivan.

Q. Wouldn't there be a memorandum on the report showing it?

A. It cannot show both at the same time.

Q. Why can't it?

A. You mean their method of taking a sample?

Q. No, I say wouldn't the report show?

A. Oh, I don't know anything about that. **[1630—1567]**

Q. The report would show the amount of moisture content at the time of the delivery in San Francisco;

(Testimony of E. E. Somermeier.)

there is no question about that, according to their method, is there?

A. According to their method, yes.

I have been in consultation with Professor Folsom lately. *Everything* he comes to town he calls around to see me. I received a preliminary report on his work some two months ago before I came west this last time. I have received no other report from him except verbally. I have visited him at Stanford and have investigated his method of work and called it good, but I have not otherwise acted with him. I have given advice as to the method in which he should carry on his examination.

Q. And have you stated to him that it would be desirable with his court demonstration to take a test of very fine coal as the presence of 1 or 2 per cent dust, if thoroughly wet, would account for a very appreciable moisture increase? A. Yes, sir.

Q. You have given him that advice, haven't you?

A. Yes, sir.

Q. You have advised him in making the test to get the very finest kind of coal, because that would show the highest degree of moisture in the coal?

A. I have advised him to show what the dust would hold.

Q. And didn't you ask him to make such a demonstration in Court, using the very finest kind of coal?

A. To show what the dust would hold.

Q. On the ground that that would account for a very appreciable moisture increase?

A. No, sir; because the amount of dust was always

(Testimony of E. E. Somermeier.)

a small amount. **[1631—1568]**

Q. You have been advising him right along as to how he should proceed for the purpose of presenting a pretty good case to the jury, have you not?

A. I have been advising him how to get the facts before the jury—not necessarily to get the facts before the jury.

Q. What do you say, Professor? Have you written him any letters containing your advice as to the method of his examination, and as to the method of presenting the defendant's case to the jury?

A. No, sir; except that one test. I advised him that that one test was a good demonstration test as to the possibility of coal to absorb moisture.

Q. Did you explain to him that he should make a court demonstration? A. No, sir; I did not.

All the samples of Nanaimo coal that I tested were low in sulphur.

My agreement with the Western Fuel Company as to compensation was that I was to get $25 a day for my time and my expenses. I was not paid any retaining fee. I have had my traveling expenses to San Francisco and to Nanaimo.

Redirect Examination by Mr. OLNEY.

The document which is here shown to me is the final report supplemental to my preliminary report, portions of which Mr. Sullivan read.

(Said document was here introduced in evidence as Defendants' Exhibit II, the understanding being that Mr. Olney would later read portions of it to the jury.) **[1632—1569]**

(Testimony of E. E. Somermeier.)

There are other publications on this subject of oxidation issued by the Government, namely, Experimental Work of the Chemical Laboratory, Bulletin Number 323 of the United States Geological Survey, has work on oxidation, on page 22, showing, among other things, increases in weight due to oxidation of samples stored in the laboratory. This bulletin is a reprint of Bulletin 28 of the Bureau of Mines. The United States Geological Survey also published Bulletin 382, entited: "The Effect of Oxygen in Coal," which was subsequently republished by the Bureau of Mines as Bulletin Number 29.

The method which I used in taking the two samples from the mines at Nanaimo for the purpose of obtaining an idea as to the moisture content of the mine is the same method as is employed by the Bureau of Mines of this Geological Survey in making tests on mines, which they have pursued in practically all of the mines in the United States.

The subject of oxidation of coal has been studied for a good many years, but in the last few years there has been a great deal more attention paid to it.

BE IT REMEMBERED that thereupon the following testimony was given and that the following proceedings occurred:

Mr. OLNEY.—Q. You have stated in response to a question of Mr. Sullivan's that your conclusions on oxidation are conservative. How do your figures and observations compare with those of others?

Mr. SULLIVAN.—We object to that as incompetent.

(Testimony of E. E. Somermeier.)

The COURT.—The objection is sustained. He has testified to what he has done as a chemist, and he has given the actual results found by him. **[1633—1570]**

Mr. OLNEY.—That is in response to a question put by them, if your Honor please, where they were questioning the conversatism or the accuracy of his figures which were based on his general observation. Now, I take it that he has the right to compare his ideas and his results with those given by other people, according to his knowledge.

The COURT.—Under the same conditions, and doing the same work, or operating on the same thing, and at the same time, perhaps, that would be true, but then you would not get any further than you have with his results and his experiments. It is just the same proposition of supporting one expert by showing that his result corresponded with the result reached by another expert who is not here.

Mr. OLNEY.—No, your Honor, but the witness is here testifying as an expert, and he says: "I got these results, this is in my opinion in this case." Now, when he is asked whether or not he is sure of that opinion, whether or not it may be too great or too little, then certainly he has got the right to say, and we have the right to bring it out on redirect examination, that his opinion, as compared with the opinion of other men, other people in the same line, the general consensus of opinion of investigators in this matter, is on the conservative side.

The COURT.—I don't think so; the objection is

(Testimony of E. E. Somermeier.)

sustained. He has told us what he has done, what results he has obtained, and how he obtained them, and he has stated to us that this is conservative.

Mr. OLNEY.—We note an exception. **[1634—1571]**

Q. Mr. Somermeier, it appeared in evidence that the car at Nanaimo which had stood out in the rain for two or three days and remained for some six weeks exposed to the sun, had gained in weight some small percentage. Will you compare the conditions that were found in that railroad car, for the holding of moisture, with the conditions which would prevail in a storage pile?

A. The conditions of retaining moisture would not be so good as in a storage pile, but approximating that toward the middle of the car; the storage pile would be much larger, and would have a greater capacity even than the car.

Mr. OLNEY.—Q. Have you observed any difference between these coals and other coals you have examined, with regard to oxidation; in other words, would the observations which you have made on other coals, in connection with the subject of oxidation, be a reliable basis for an opinion concerning the oxidation of these coals? A. Yes, sir.

Recross-examination by Mr. SULLIVAN.

Mr. SULLIVAN.—Q. This report, Bulletin 29, issued by the Bureau of Mines, on the effect of oxygen in coal, which you have handed to us, I wish you would point out in this bulletin any place where reference is made to the fact, if it be a fact, that the

(Testimony of E. E. Somermeier.)

weight of coal is increased by oxidation.

A. On page 68. **[1635—1572]**

"It is probable that in the sub-bituminous coals, and more especially in the lignites, oxygenation begins immediately after the coal is blasted from the face in the mine. Zincken quotes Bischoff to the effect that the brown coal of Putzchen absorbed 11 per cent of oxygen from the atmosphere in eight days," and so on.

Q. Just point out there in that book any word or any sentence which declares that the oxidation of coal results in increasing the weight of the coal?

A. I thought that was direct.

Q. That is a mere statement as to the effect of oxygen in coal.

A. It says it absorbed 11 per cent of oxygen from the atmosphere in 8 days.

Q. Just show me anywhere in that book which says that oxidation in coal results in increasing its weight?

A. That statement there says it.

Q. Is that the only statement you can refer to?

A. I can read the rest of the page if you desire it.

Q. Show me anything in that book which shows that coal increases in weight by oxidation?

A. "More recently Stremme and Spate have shown some absorption even by sapropelic coals standing for various periods in the museum. It should be noted, however, that the sapropelic or alga coals are very much less susceptible to oxidation on exposure than are the more bituminous coals, which are humic.

"It is not impossible that some of our peats will

(Testimony of E. E. Somermeier.)

be found to assimilate still greater quantities of oxygen on exposure. This question, as well as that of the immediate loss of volatile in freshly mined lignites or sub-bituminous coals, is one deserving careful examination. The results **[1636—1573]** of the very interesting as well as important, investigations recently published by Parr and Hamilton, show that deterioration is strongly marked and rapid."

Q. Can you point out anything in that book which says, especially under the title, "Weathering of Coal," that coal increases in weight by reason of oxidation?

A. As I interpret chemical language, Mr. Sullivan, what I have been reading is nothing else than that.

Q. This coal at Nanaimo is not sub-bituminous coal, is it? A. It is bituminous.

Q. It is not lignite, is it?

A. It is bituminous coal.

Q. Is that all that you can find in that book which indicates or states that coal is increased in weight by reason of oxidation?

A. No, sir; continuing on the same page further:

"It may not be over-hazardous to estimate that some of the published calorific values are as much as 200 calories less than the efficiency of the coal in the ground." Determination of calorific value is equivalent to an increase in weight, approximately.

Q. Is not the calorific value of coal affected by moisture? A. Yes, to the extent—

Q. (Intg.) And if the moisture of the coal were increased from 3 per cent to 10 per cent, the calorific

(Testimony of E. E. Somermeier.)
value would be proportionately affected?

A. Proportionately less.

Q. Even if no oxidation took place at all?

A. Yes.

Q. Can you find a single sentence in that book that positively shows that oxidation increases the weight of coal, in so many words?

A. The whole stuff that I have been reading says that. **[1637—1574]**

Q. That is your interpretation of it?

A. And my interpretation is the interpretation of a man who knows his subject, I think.

[Testimony of Ernest Kroder, for Defendants.]

ERNEST KRODER, a witness called for the defendants and sworn, testified as follows:

Direct Examination by Mr. KNIGHT.

I am master of the S. S. "Thor" and for a number of years last past I have been engaged in carrying Japanese and Australian coal from Japan and Australia, respectively, to this port. They determine their bill of lading weights of coal in Japan by the capacity of their lighters. They have a water line on their barges and they know how much coal it takes to sink down to that mark. You can reach an approximate estimate of the quantity of coal laden on the S. S. "Thor" by knowing her carrying capacity.

Cross-examination by Mr. SULLIVAN.

I have been master of the "Thor" since July, 1913.

I have made two trips to Japan in her. Prior to that I made two trips to Japan in the "Titania," and

Testimony of Ernest Kroder.)

I have been to Japan in other boats but not in the coal carrying trade. I took the "Titania" in 1907, and kept her until 1913, when I took the "Thor." On the other vessels I took bunker coal at Japan. In getting the bunker coal it was the custom to weigh every tenth bag ashore and take the average for the rest of the bags. **[1638—1575]**

The chief engineer oversees this weighing. When it comes to taking a cargo of coal, however, as distinguished from bunker coal, the bags are simply filled at great piles of coal ashore and dumped into the lighters. The bags are not weighed. We know how much coal we are getting by the capacity of the lighters, but of course that is an estimate, not correct according to the pound. The lighter, of course, containing a certain quantity of coal, sinks down to a certain depth in the water, and there is a mark there indicating the full capacity of the lighter. I should say that we would come within a ton or two of the absolutely correct weight in this way. They take the weights by the capacity of the lighter; the bags are not weighed before they are dumped into the lighter.

[Testimony of David M. Folsom, for Defendants.]

DAVID M. FOLSOM, a witness called for the defendants and sworn, testified as follows:

I reside at Stanford University and am associate professor of geology and mining in that institution; I have held that position for about 3 years, and am by profession a mining engineer, having received my technical training at Stanford University and the Columbia School of Mines. Having graduated from

(Testimony of David M. Folsom.)

those institutions, I spent a few months traveling around and visiting mines through Montana and then entered the employ of the Boston & Montana Copper Company, at Great Falls, Montana, for about five years, and until I returned to Stanford. I have now been at Stanford since March 1, 1910.

I have been employed by counsel for the defendants in this case to make certain experiments to determine the effect of moisture and oxygen upon the weight of coal.

I made one test upon a large lot of coal in the bunkers at Folsom Street, in this city. The date when the test started was August 14, 1913. One of the bunkers of the regular series at the Folsom Street yard of the Western Fuel Company was divided by a partition and into one of the pockets **[1639—1576]** formed thereby a lot of approximately 150 tons of coal was weighed. The weighing was done by the Government customs official, and the coal was dumped from the cars into this pocket in the usual way. It was Australian coal from the S. S. "Rothley." The weighing done by the Government weigher was in the regular discharge of the vessel. The cars were watched to see that the weight taken applied to the particular car; the contents of which were placed in the bunker. When the coal was placed in the bunker on August 14, 1913, it weighed 331,740 pounds. It was sprinkled for four nights over the surface by the use of a 5 gallon garden watering can. The total amount of water added to the coal in this way was equal to 5% of the weight of

(Testimony of David M. Folsom.)

the coal, or about 16,667 pounds of water. This would be approximately equal to the amount of water which the bunkers would have received from 5 inches of rain over the area of coal in the bunkers. The sprinkling was at the rate of 500 gallons per night, and extended over a period of from 8 to 12 hours as a maximum; the sprinkling was done rather slowly. After four nights of this sprinkling, the coal was weighed out in the same manner, as is followed in ordinary commercial transactions, over the wagon scales at Folsom Street by Mr. Miller. I observed all the weighings. The coal weighed out at 339,270 pounds representing a gain of 7,530 pounds, being 2.27% of the original weight. We took every precaution to see that nothing besides the water was added during the interval of this test. I had two students from the Geological Department at Stanford on watch at the bunkers all the time—they were on 12 hour shifts. Thus, this particular pocket or bunker was watched day and night from the time we started. As to the scales, I had an inspector sent down from the Fairbanks Morse people before we started, and got a report from him in writing that the scales **[1640—1577]** were correct.

I made a second experiment of the same sort. On August 18, 1913, 323,170 pounds of coal were weighed into the same pocket of the bunker as was used in the last experiment. This experiment was repeated in exactly the same manner except that the lot of coal we used in this case, being taken during the cleanup of the ship, was coarser and had more lumps in it

(Testimony of David M. Folsom.)

than the first lot. This coal weighed out after 4 days at 329,930 pounds, a gain of 6,760 pounds, or 2.09%. Our idea was to get an approximate idea of the effect of fineness or coarseness in the coal on the increase in weight by moisture content. As the coal went into the bunker it was very dry and dusty. When it was taken out it was dry on the surface so that a casual observer would not notice any wetness or drops of water, with the exception, perhaps, of the fines which appeared moist.

The next experiment was for the purpose of determining the moisture content of Australian coal as it comes in to this port. I took some 10 or 12 samples, usually two per day, from a vessel discharging here. The samples were actually taken from the cars or the top of a bunker just after a car had been dumped. Some of these samples were analyzed and the moisture content determined in the chemistry laboratory at Stanford, and some of them were sealed and delivered to Professor Parr and Professor Somermeier for their determinations. The average moisture content of the samples which were analyzed at Stanford was 2.76%.

My next test was to determine what the coal would increase in weight by the addition of moisture over a considerable period of time. The coal used in this test was Richmond **[1641—1578]** or Australian coal. (Plate 1 at this point exhibited to the jury was, according to the witness, a graphic representation of the results of such test.) The test represented on plate 1 was made on coal taken here in the

(Testimony of David M. Folsom.)

city from the bunkers just after a steamer had been unloaded. The coal was placed in a galvanized iron can in the bottom of which 20 holes were punched about a quarter of an inch in diameter and over the bottom a layer of porous canvas was laid which would allow the water to drain out rapidly, but would retain the fine coal. Before the test was started the canvas cloth and the can were thoroughly wetted and weighed to get the tare. Then coal weighing 57.2 pounds were placed in the can. The coal was sprinkled first with an amount of water equal to approximately 10 per cent of the weight of the coal—five to ten per cent, and weighed a few hours after sprinkling; and then weighed a day after sprinkling. The time to which the coal was subjected to wetting is represented by a line along the bottom, on which the days are indicated, and the months. After about 24 hours following the sprinkling of the coal, water was added to the can of coal, in sufficient quantity to cover the coal, that is, a pailful of water was poured in, and the weights were noted a few hours after, and several days after.

A great many weights were taken which do not show on plate 1. The first percentage of water added increased the weight of the coal a little over 2.1%. The explanation of the percentage lines on the diagram is as follows: The first red line, the lower red line represents the original weight of the coal; above that, these lines represent percentage of gain in weight; the heavy red lines at inch intervals represent half a per cent; the light red lines a tenth per

(Testimony of David M. Folsom.)

cent of gain. The irregular black line running from one circle to another represents the line of change of weight. Two to three hours after the coal had been covered with water most of the water had drained out, but there was still 4.4% retained and the lot weighed 59.7 pounds. After three days of draining out, the lot was reduced to 59.2 pounds, and from that time on the weight remained practically **[1642—1579]** constant until the next weighing, which was on October 2d. The interval between wettings would be about one week. Two days after the coal was wet by covering it with water it reached a constant weight, that is to say, the coal would first show a gain of from 5 up to 9 per cent, then part of the water would drain off and part of it would be retained, and the draining being completed in about two days, there would be practically no further loss of weight, in other words, the weight would remain practically constant. After the second wetting, after the second soaking, when water was added a second time, it will be noted that the increase of weight was greater than after the first lot of water had been added. On taking the weight a few hours afterwards, it gained up to 60.1 pounds, 5 per cent increase over the original weight of the coal. Water drained out until on the 3d of October it weighed 59.6 net weight. This draining continued until about the 6th of October, and then for three days the weight remained constant. Water was added a third time, and the increased weight amounted to nearly 6 per cent—5.9 per cent; this weighing was taken very soon

(Testimony of David M. Folsom.)

after the water was added, probably about 2 hours was the usual time which was allowed before any weight was taken; after two hours, it gained about 5.9 per cent. Two more hours, it had dropped down to about 5¼ per cent; in 24 hours from the time it was covered with water it dropped down to 4 per cent, and in about two days it had reached a constant weight, a weight of about 59½ pounds, which represents a gain of 4 per cent. This operation of soaking the coal was repeated until the 10th of November, and after the 10th of November, the coal was exposed to air drying, being allowed to stand without any further addition of water, until the 24th of November. **[1643—1580]**

This can was not out in the open air. A loose cover was placed over it. The experiment was an attempt to imitate the conditions on the inside of a pile of coal, and the lot of coal being so small the surface evaporation would have played an important part, had it been entirely exposed to the currents of air. That would not be the case with the inside of a pile of coal. The evaporation would there probably run down a foot or so. After the 24th of November the coal was exposed to the rain out in the open, the covering being removed and left off until the second of January. During that period there was a little over 7 inches of rainfall.

This chart or plat 1 represents the time in which the rainfall—that is the steps up in this chart represent the rainfall to which the coal was exposed. The horizontal lines represent simply the weather condi-

(Testimony of David M. Folsom.)

tions, the solid block represents night, the shaded portion of the horizontal line represents foggy weather, clouds, as shown in the legend, and the open square spaces represent days when the sun shone and when the coal would be exposed to evaporation; the change of weight from the rainfall of course is fluctuating, generally depending upon the amount of rain at the time. Usually, in the first period the rain fell at night and it was impossible to get an observed weight immediately after the rain, as in this case here, the weight was not taken until several hours after the rain was over.

Q. At that time did the chart show an increase in weight of the coal?

A. It shows the weight increased after the rains, and the rain drained off, water due to rainfall drained off, in a similar manner. During the **[1644—1581]** period from November 29 until about the 21st of December there were no heavy rains; there were a few showers; about .14 of an inch on the night of the 12th of December and a little rain on the night and morning of the 19th of December, but the coal for the most part, for a period of about 15 days, was exposed to clear weather in the daytime, or on sunshiny days, and a certain amount of evaporation took place, which decreased the weight and brought down the gain to a little over 4½ per cent, to 4.35 per cent.

Q. From November 29, according to this chart, to December 12, a period of some 13 or 14 days, was there any rain?

A. No rain at all, no observed rain; there was a

(Testimony of David M. Folsom.)
little rain here, but it was so small it could hardly be measured; I think it was .01 of an inch.

Q. What was the behaviour of the coal during that period in regard to weight?

A. During the first part of the period it lost weight, and from the 5th of December to the 12th it remained at a constant weight, with no appreciable loss.

Q. How much gain over that at the beginning?

A. 4.35 per cent. With the beginning of the rain the coal increased in weight again; the weights at such times were taken only about every day and for a while no attempt was made to take the peaks, that is, to get the high points immediately following heavy storms. For instance, this storm represents over an inch of rainfall during one night, and the weight of the coal was not taken until 2 o'clock in the afternoon, when it had several hours in which **[1645—1582]** to drain In the heavy storm on the night of the 31st of December, an attempt was made to study the exact rise and fall of this weight, and the last weight which was taken was at 10 P. M. December 31st. The net weight of the coal was 61½ lbs., which represents a gain of 7½ per cent over the original weight of the lot.

* * * * * * * * *

A. 7½ per cent. This rain stopped about midnight of the 31st at Palo Alto. The next weight taken sometime about 11 o'clock in the morning shows a drop down to 60.4 lbs., a gain of 5.6 per cent. Another storm on the 2d increased the weight of the coal somewhat, and there was a drop after that to

(Testimony of David M. Folsom.)

60.4 lbs., and to 60.3, and then finally 60.2. The time interval represented after the last storm is greater than that implied by the length of the chart. It represents a period of approximately two weeks. Now in this period the coal was taken inside and the can covered again, and the coal simply allowed to drain out in an ordinary manner, and at the end of approximately two weeks, or 13 days, the coal weighed 60.2 lbs., a gain of 5¼ per cent from the original weight. At the end of the test this coal was screened through a one-half inch mesh. The amount of fine material under one-half inch was 22% of the total sample. The moisture determinations on this coal before it had been placed in the can showed 2.2% moisture. The zero mark on the platted diagram represents the original weight of the coal with 2.2% of moisture already in it. The final moisture taken from a sample at the conclusion of the test was 7.3%. This coal was screened and the fine and the coarse separated, the moisture determined on. The fines contained 13.83% of moisture, while some of the lumps which were taken out contained 4.44%. The increase in weight as shown by the differences indicated by the analyses was 5.1% on the total sample. By actual **[1646—1583]** weight I got 5.25.

Q. Now, I note on this diagram, Professor Folsom, that in the table as you add water heat, as you call it, that is the weight of the coal taken a few hours after the water is added, and of course when it would weigh most, gradually goes up, and likewise the weight of the coal after it has been allowed to

(Testimony of David M. Folsom.)

drain for a considerable period goes up after successive wettings. Has that fact any significance, and if so what is it?

A. I think the fact has this significance, that while—

Q. (Intg.) Will you explain to the jury just what the fact is? I am calling your attention to it; I might not have made it very clear.

A. Well, the most significant thing about the whole test is that beginning with a 2 per cent gain in weight following sprinkling and 3.6 per cent gain following the first soaking, that each additional soaking meant an additional gain in weight, fractional in many cases, but in every case a little additional gain weight. Now, this is due apparently to the packing of the fines in the coal as the water passed down through the coal, and in draining out and in packing these fines until they were a more or less impervious layer; also it was due to the weathering of the coal in repeating the dryings and wettings it was subjected to; the fine cracks in the coal open up, and more pieces of coal chip off the large lumps, making a greater amount of fines, and in small part it is probably due to the large lumps of coal actually absorbing and retaining a certain amount of this moisture; for **[1647—1584]** each addition of water there is additional weight, even after a period sufficient for draining has lapsed. I may say that this can is approximately 13 inches in diameter and about 20 inches in height, and it represents a depth of only about 20 inches of coal, through which the coal could

(Testimony of David M. Folsom.)

drain very readily. At the close of the test you will note that a good deal of the fines were concentrated in the lower part of the can, due to the action of the water in washing the fines down and packing it.

Q. What would be the effect of that concentration as to giving up moisture?

A. The fines which are packed hold the water mechanically for a long period of time, not only hold it mechanically, but prevent the air coming in and causing evaporation of the water so held.

Q. That is, it would increase the difficulty of the coal giving off moisture by draining or by evaporation? A. It would.

Q. In each case, after you have added water does the coal come back to a position and to a weight at which it will practically lose no further weight by reason of evaporation or drainage?

A. It does; or if a loss, it would be a small loss, but these weights were taken to the nearest two ounces and over a period here of two weeks, from November 10 to November 24th. The last twelve days of that there was only a loss of about 2 ounces out of a total weight of 60 lbs.; that is a loss of about one-tenth of one per cent.

Q. Would or would not that indicate that the [1648—1585] coal would hold that amount of moisture indefinitely?

A. It indicates that it would hold this moisture indefinitely; that is, it might lose a fractional percenage; if it were left in there for a year you expect the

(Testimony of David M. Folsom.)

flow to drop off, part of a per cent.

Q. And these points at which you hold moisture in this manner indefinitely, as I understand you, come gradually up, gradually higher with each successive wetting?

A. They do. Each successive wetting added a certain weight to the coal.

This test began September 23, 1913, and ended January 15, 1914. Up to November 10, 1913, water was added artificially. After that no water was added except by rain. From the 25th of November until the 2d of January the can was out in the open air. Then it was taken into the laboratory and allowed to drain out in a large room there in which the experiment was performed.

My next test was similar to that last described, except that the coal used was Wellington instead of Richmond or Australian, and that the period of exposure was shorter.

The test was made in exactly the same way on a lot of 61.9 lbs. of coal sprinkled first, about 5 per cent moisture. It weighed in 24 hours, 60.9, a gain of 1.6 per cent. I then added water sufficient in quantity to cover the surface of the coal, and this water drained through, the maximum gain being 4 per cent, and then dropping down to 3.2 per cent; practically a constant weight maintained for four days; the test started on October 9th and on October 18th, after the second wetting **[1649—1586]** the second soaking, after draining, the coal had gained 3.5 per cent. Then the galvanized iron can was

(Testimony of David M. Folsom.)

placed in a larger vessel in which there were no holes punched and the whole coal was covered with water and allowed to stay that way 38 hours approximately; after that a small galvanized can was taken out and water allowed to drain out; it drained out very rapidly; the coal maintained, or retained 4½ per cent of its original weight.

Mr. SULLIVAN.—Q. What date was that? What was the weight when that water was put into it?

A. Before the water was put into it, the third soaking, the weight was 64.4 lbs. After soaking for 48 hours it weighed 65 lbs., and dropped to 64.8 and 64.7.

Mr. OLNEY.—Q. Before this weight of 65 lbs. had been taken was it allowed to drain at all after it had been soaked?

A. It had been allowed to drain—I will have to look into the detail note-book to tell you how long; it was allowed to drain several hours before this weight of 65 lbs. was taken.

Mr. SULLIVAN.—Q. When was that water put into it—what date?

A. On the 25th of October; it drained until some time on the 27th.

Q. It went up to 65 lbs., you say?

A. Yes, that weight was taken several hours after it was allowed to drain.

Mr. OLNEY.—Q. After that what was the behavior of the coal, after this soaking?

A. Then the water was added again on the 3d of

(Testimony of David M. Folsom.)

November and allowed to stand for a week; this presents **[1650—1587]** the time between the 3d and 10th, because the weights were not taken every day; it came to 64.8 lbs.; then water was added again on the 10th of November. On the same day on which water was added to the lot of Richmond coal that was being tested alongside of it. After the 10th of November no water was added by artificial means, and the coal was allowed to drain until the 24th of November, a period of two weeks, and it weighed at that time 64.7 lbs., which represents a gain of a little over 4½ per cent. It was then taken out in the open and exposed to rain until the 6th of December; during that time there was about 1.3 inches of rain in three storms. This rain occurred at night, and the weights were not taken until after the coal had been allowed to drain some hours, but it showed some increase; the highest recorded weight being 65.1.

Q. That would equal 5 per cent?

A. That would equal a little over 5 per cent, and the final weight after draining out in the open, with the covering of the can off from the 29th of November until the 6th of December was 64.6. This weight you will observe was two days constant. It represents 4.35 per cent gain in weight from the original weight. This coal was screened at the end of the test and found that about 30 per cent of the 62 lbs. which was taken as a sample was fine coal under ½-inch diameter; the coal had been sampled before going into the can and had a moisture content of 2½ per cent. The sample of coal taken at the conclusion

(Testimony of David M. Folsom.)

of the test had a moisture of 6.8 per cent which is a gain of 4.3 per cent; a gain in weight per cent of 4.35 per cent. The moisture **[1651—1588]** in the screenings was about 12 per cent; the fine coal retained much more moisture than the coarse coal.

Considering that the Wellington coal used in this experiment was exposed for a shorter time than the Richmond coal used in the last above-described and preceding experiment the coals behaved much the same. The increase by successive wettings is just in accord with the increase by successive wettings in the Richmond coal. The fine coal packed in just the same way and contained much more moisture than the lump coal. There is no observed difference in the behavior of the two coals when subject to artificial wetting or when subject to natural wetting through rain. (The witness used a plat in explaining this experiment to the jury.)

My next experiment was also on Wellington coal. It shows exactly the same results as the last experiment, except that the coal was exposed for a longer period of time. There was about 27% of the fine coal, and the gain was 4.1%. This lot of Wellington coal was exposed from October 12, 1913, until December 14, 1913. It had a longer period of air drying out in the open at the conclusion of the test. From November 24th until December 12th, there was no rain at all; then there was a shower of .14 of an inch on the night of the 14th. The increase in the coal before the last shower was 4.1%. At the end of

(Testimony of David M. Folsom.)

the experiment the can was exposed to the weather and the sun. The cover was off. (This experiment was also illustrated to the jury by a plat.) **[1652—1589]**

My next experiment was a test of coal exposed only to the rain, and not subjected to artificial wettings. I used a sort of receptacle, namely, a can with holes punched in the bottom and a layer of canvas laid over them. This can was placed out in the open without shelter from the rain on the night of October 31, 1913, and rainfall observations were taken along at the same time that the weights were taken. 56.9 pounds of coal were placed in the can. The coal varied in weight with the weather, increasing following rain and draining out somewhat after rain, especially on days when it was very clear and the sun was shining bright, and it was warm; during foggy days, which are indicated on this chart by a vertical etching on this rain chart, there was very little loss of weight, you could not observe any loss in weight; in fact, on some heavy foggy nights, there was a very slight gain of perhaps an ounce in a lot of 57 pounds of coal, the fog only affecting the surface. Following these rains, it rained here less than an inch and the coal gained about 1 per cent; after a sunshiny day, it dropped down to about half a per cent; following another rain of .3, it built up to 57.6 pounds, a gain of 1.75; and then for a period between about the 4th of November and the 10th or 11th, there was no rain, and it dropped back to about half a per cent. Then there were several showers at

(Testimony of David M. Folsom.)

night, which amounted to about .7 or .8 of an inch, and it built up to 57.9 pounds, a gain in weight of 1.75. It drained off partially until the 17th, and then heavy rains again came in, or rains which amounted to a little over an inch, and that built it up to 58.1 pounds, a gain of over 2.1 per cent; then it drained in fair clear weather **[1653—1590]** warm, for four days, and dropped down to an apparently constant weight, no change in weight between the 21st and 24th, although the weather was fair, at that time, of 1¼ per cent, 57.6 pounds. Then storms built it up again to a gain of nearly 2½ per cent. Then, between the 29th of November and the 12th of December, there was this period of fair weather, during which the coal remained at practically a constant weight of about 1.7 per cent gain. Then there were some foggy nights, which had a little effect on it, and then a shower of about .01 of an inch, and then a shower on the night of the 12th of .14, the coal gained to over 2 per cent in its original weight, and then there was a period of draining until the 18th. On the morning of the 19th this coal was taken out, just after a shower of perhaps .2 of an inch, and the final gain was 2.2 per cent —about 2.3 pounds out of 59 pounds.

Twelve per cent of the coal was screened under a ½-inch mesh. The final moisture on the entire lot of coal showed an increase of 2.2%. The percentage gained in moisture in the lump coal was 1.06 and in the fines 9¼%. If the fines in this sample had amounted to 25% instead of 12% of the whole, I

(Testimony of David M. Folsom.)

would say, judging from the other tests that I have made that the gain would have been from 3 to 4% in the whole lot.

My next test also involved the exposure of coal to the rainfall without any artificial wetting. The chief difference between this and the former test was that I used a more considerable quantity of coal. I had a bin 4 feet square and 5 feet high and alongside of it a galvanized iron can 10 inches in diameter and 5 feet high, and containing 115 pounds of coal. There were 20 quarter-inch holes in the bottom of the can with **[1654—1591]** a layer of canvas over it. The wooden bin contained 4200 pounds of Richmond coal. This experiment is represented on my plats 4 and 5. The bin and the can were exposed to the air between November 11, 1913, and January 2, 1914. There was an increase in weight in the can during that time of 1.65%. There was an increase in weight in the bin of 2.15%. The bin was not water tight, being made of coarse, rough lumber. The can contained 5% of screenings only, that is to say, it was practically all lump coal. The bin, on the other hand, contained 12% of fine coal. I did not in this experiment take daily weights. I did not know before the test what percentage of fines I had in either lot, and it was only at the conclusion of it all on the screening that I found the can was a coarser coal, a cleaner coal than the bin. They were both filled out of sacks, and I didn't note the percentage of fines at all. If the bin had contained 25% of fines instead of 12%, I should judge, from the other experiments that I made later

(Testimony of David M. Folsom.)

than these, with the same amount of rain, that the increase in weight would have been in the neighborhood of 3½% to 4%. It might have amounted to more than that, but it would at least have been that much. I used for the screenings in this case, as in the previous experiment, a half-inch mesh.

Next, I made a series of 30 or 40 tests to observe the ratio or extent to which coal would increase in weight by the addition of moisture, according to the percentage or proportion of fines it contains. These tests extended altogether over a period of months. After the tests the coal was screened and the amount of material under one-half inch determined. The coals which had a high percentage of fines gained much more in weight when subjected to rain or when subjected to artificial wetting, than the coarse lump coal. Coals that had 5% of fines would only **[1655—1592]** gain, as a maximum, about 1½ per cent, or as shown here, 1.6, while coals that had 30 per cent of fines would increase, as a maximum, 5 per cent, after repeated wettings. The extent to which coal will increase in weight varies directly in proportion to the fines. My chart No. 7 shows in graphic form the relation between the two. These red circles on this chart represent tests which were made in the laboratory to determine the maximum amount of gain which could be expected with any lot of coal, with a given percentage of fines, calling fines arbitrarily all coal under ½ inch in diameter. I might mention that the openings in the screens on the Folsom Street bunker are ⅝ of an inch apart, that is to

(Testimony of David M. Folsom.)

say, a little over a half inch. **[1656—1592½]**

The coal was subjected to an artificial wetting until it had gained approximately the maximum amount that could be expected of it, and further wetting would not increase the weight.

The gains that I am about to give represent the maximum gains of Wellington coal according to the percentages of fines, that is to say, the coal was subjected to wetting until it gained approximately the maximum amount that could be expected of it, so that further wetting would not increase that weight. Clean coal, that is, lumps without fines, would gain .8 of a per cent; coals with 5% of fines would gain 1.5%; coals with 10% fines, 2%; with 15% fines, 2.6%; with 17% fines a little over 3%; with 22% fines, 4%; with 27% fines, 4.6%; with 30% fines, 4.7%; with 40% fines, 7%; with 45% fines, something over 71%; with 59% fines, 8%. A small lot of Wellington fine coal all under ½ inch in diameter gained a little over 15%. These figures that I have given here are not the peak weighings, as shown on the other diagrams immediately after the water is added, but represent the valleys, and in no case are they less than 4 days' draining. The result would be that on coal containing 5 per cent of fines, there would be a permanent increase—what you may call a permanent increase of moisture to the maximum amount of approximately 1½ per cent, and it would retain that at least several months—the period of time during which these tests were conducted. This coal was not exposed to a very long draining, being placed in

(Testimony of David M. Folsom.)

a large canvas which was shaken and twisted around so that the water could be squeezed out of the canvas in an attempt to dry it out. That would be a case of 100% screenings. In general then it may be said that the extent to which coal would increase in weight varies directly with the proportion of fines **[1657—1593]** that it contains. I now present the table designated table 1, indicating the percentages of increase in the weight of coal, in proportion to the amount of fines contained in said coal where the coal is subjected to artificial wettings.

This is a tabulation showing the effect of size of coal on the maximum gain in weight on the coal that has been wet by artificial means, showing the amount of coal taken in each lot in the test, the kind of coal, whether Australian or Nanaimo coal—the Wellington, the percentage of fine coal, using the same arbitrary assumption **[1658—1593½]** that coal under ½ inch in diameter is considered as fine, and the gain in weight after repeated soaking or wetting; that is, taking commercially clean coal, lump coal, which is sold commercially here in the city as lump coal, but contains a small amount of dirt and fines, and after wetting it it very quickly reaches its maximum; it is in the second wetting it reaches a maximum gain of 1½ per cent. Coal, which at the completion of the test was screened and showed 10 per cent of fines, showed an increase after the first wetting, and after four days draining after the first wetting of 1.56 per cent; after the second soaking 2 per cent and after the third soaking 2 per cent. 15

(Testimony of David M. Folsom.)

per cent of fines, the first soaking was 1.62, the second 1.83, the third 2.44, the fourth 2.63, and the fifth 2.81. These two are Wellington coal, tests 19, 13 and 20, are all Wellington coal. Test 14 is Richmond coal. 17 per cent of fines under ½ inch, as far as the test was carried, showed a gain of 3.25 per cent.

Q. That was after the third wetting?

A. Yes, sir.

Q. And after four days' draining?

A. Yes, apparently it had not reached its maximum. With 22 per cent fines, in the Richmond, after the fourth wetting it showed 4 per cent; after the first wetting it only showed 3 per cent. With 27 per cent fines, in Wellington coal, after the first wetting it showed 3.1 per cent increase after four days' draining; after the fifth wetting an increase of 4.55 per cent. 30 per cent fines, after the first wetting showed an increase of 3.2 per cent; after the fifth soaking, 4.7 per cent. Test No. 10 was made on a lot of commercial screenings, a sample taken here in the city; the coal contained practically no dust; it was coal which ranged in size from just under one-**[1659—1594]** half inch down to about the size of wheat, and with very little dust in it, some dust, but it was fairly free from dust, that showed a gain of 3.08 per cent after the first soaking, and after four days' draining, and 4.74 after the fourth soaking.

Q. Would the presence in commercial screenings of coal as fine as dust have made any difference?

A. Yes, it would.

(Testimony of David M. Folsom.)

Q. In what way?

A. The fine dust, from the test which I made on it, would hold as high as 40 or 50 per cent of its weight in moisture; and the small amount of dust in this would have an appreciable increase on the gain in weight.

I have another table or plat designated table 2, designed like table 1, to indicate the percentage of increase in the weight of coal in relation to its proportion of fines, except that this coal was subjected to natural rainfall instead of, like the coal in table 1, to artificial wetting.

A. This is a tabulation of a number of tests made of various lots of coal in amounts ranging from 53 lbs. up to 4,200 lbs.; the coal was exposed simply to the rain, no artificial wetting whatever. The weight of the sample is given, the kind of coal, the percentage of the coal under ½ inch, the time which it is exposed, the amount of rainfall during that time and the time of draining after the last shower before this final determination of gain was made; and also the highest recorded gain during that period of time; that is, on these small samples the line of recorded gain corresponds to the highest peak in some of these preliminary plates that have been shown. The coal with only 5 per cent of fines, exposed 35 days, and with 3.8 **[1660—1595]** inches of rain, but with 16 days of fair weather after the last rain, gained .95 of one per cent.

Q. That was with 116 lbs.?

A. Yes, sir; 116 lbs. in a can 5 feet high. The

(Testimony of David M. Folsom.)

highest recorded gain during that time was 1.63 per cent. The same test continued 17 days, with 6 hours draining after the last shower, showed a gain of 1.64 per cent, the highest recorded gain being 2½ per cent. With 57 lbs. of Wellington coal, in a can about 20 or 24 inches in height, exposed for 40 days, with 4.9 inches of rain, and with 10 days' draining after the last rain showed a gain of 1.7 per cent, with the highest recorded gain of 2.45.

Mr. SULLIVAN.—Q. The highest recorded gain was of course during that period of exposure?

A. Yes, sir; it was not the highest gain but it was the highest recorded gain.

Mr. OLNEY.—Q. And with a drainage of ten days?

A. Ten days. That was 1.7 per cent. After that there was a rain of about .3 of an inch and a draining of only 4 hours after that, and that was 2.3 per cent. A large lot of 4,200 lbs. of Richmond coal, 12 per cent fines determined by screening to the test, exposed for 35 days, with 3.8 inches of rain, 16 days' draining, gave a little over 1.02 per cent gain in weight. It was impossible in these cases to determine the highest recorded gain.

The same lot pushed back into the bin and exposed until it had had 17 more days, and until the total rainfall was 9.67 inches, and drained about 4 hours after the last shower, that is, there was a shower in the morning and the coal was weighed beginning at about 11 o'clock in the morning and finishing **[1661—1596]** about 2 o'clock in the afternoon, it showed

(Testimony of David M. Folsom.)

a gain of 2.15%. A lot of 53 lbs. of mixed Richmond and Wellington coal, exposed for ten days, during which time there was a rainfall of 4.68 inches, with 19 per cent of fines, showed a gain, after 28 hours' draining, that is, 28 hours elapsed after the last shower and before the coal was weighed, of 2.36 per cent, the highest recorded gain being, just after the heavy rain, 3.78 per cent. Another lot, with 20 per cent of fines, exposed for the same length of time, the same amount of rain, 28 hours' drain, showed a gain of 2.48 per cent, the maximum recorded gain during that time being 3.3 per cent.

A lot of 2,950 lbs. of mixed Richmond and Wellington coal, placed in the bin, approximately 5 feet in depth, determined after the test to have 22 per cent of fines in it, exposed for 13 days, from the second until about the 15th of January, during which time there was approximately 2½ inches of rain and drained 8 hours after the last shower, the last shower having ended about midnight and it was weighed out the next morning, it had a gain of 2.24 per cent.

A lot of 72 lbs. of Richmond coal, 30 per cent fines, exposed for 24 hours, with 7.16 inches of rain, draining for 28 hours, showed a gain of 3.83 per cent after that draining, the highest recorded gain being 5.44 per cent.

A lot of 61 lbs. of mixed Richmond and Wellington-Australian and Nanaimo coal, with 59—practically 60 per cent of fines determined by screening after the test, which was exposed for two days, during which time it rained an inch and ¼ and drained for 12

(Testimony of David M. Folsom.)

hours, showed a gain of 7 per cent, the highest recorded gain being 7.82 per cent.

This lot of coal was afterward wet down very thoroughly, with various buckets of water, put on it rapidly, and wet several **[1662—1597]** times in the course of two days, and then drained for two more days—several more days, I don't know the exact number, and it showed a gain of about 8 per cent. That is, by artificial wetting, a great quantity of water only increased this gain about one per cent, though it was not extended over any great period of time.

Q. In other words, wet by rain had brought it up nearly to saturation? A. Yes, sir.

I also had a table or plat of a test, showing the gain in the weight of coal simply exposed to the rain in January, 1914.

This test is exactly similar to the ones shown this morning, and made in just the same way, this with a can, and perforated holes and the canvas; the original weight of the sample, 71.7 lbs., placed out on the 2d of January; there was no rain until the morning of the 7th—the night of the 6th, but the weight remains constant during this time, from the 6th and 7th; then following a rainstorm of about .4 of an inch it increased in weight 1.25 per cent. Between the 6th and the 12th there was no rain and a slight drop in the weight down to where it increased was only about .7 of one per cent; and then several repeated showers between the 12th and the 14th that amounted to about 2 inches brought the gain up to

(Testimony of David M. Folsom.)

2.1 per cent after 24 hours of rain; after 48 hours of drain it still had 2.1 per cent gain; then a heavy storm amounting to about an inch brought the peak up to 4½ per cent, and after draining still had 3 per cent. **[1663—1598]** Another storm brought the gain up to probably 4½ per cent, the peak in that case was not noted, the coal was not weighed for 6 or 8 hours after the rain stopped, the rain stopped about midnight and the coal was weighed about 11 o'clock the next morning; but after two days draining following that rain there was still approximately 3½ per cent water retained in the coal. Then a heavy storm brought it up to approximately 5½ per cent of increase, and after draining it brought it down to about 3.9 per cent over the original weight. In some of the charts showing artificial wetting the point is brought out that after each draining the coal reached approximately a constant weight and that this constant weight increased with successive soakings. The same is apparently true when coal is exposed to rain that not only the peaks are built up by repeated showers and storms, but that the constant weight or the amount of water which the coal will retain after these wettings increases with each successive storm and each successive addition of water, whether by artificial or natural means. This coal was exposed for a period of 24 days and to approximately 7 inches of rain as a total.

Q. And the gain was 3.9 per cent?

A. The gain was 3.9 per cent. The original moisture content in this coal was 2.4 per cent; the final

(Testimony of David M. Folsom.)

moisture on the whole sample was 6.23 per cent; the moisture in the fine coal screened out was 10.4, and in the lot of coarse coal, about 4.44; and there was one large lump of the coal, about 10 inches in diameter, which was selected, was crushed and the moisture determined to be 3.8 per cent. The total amount of screenings was 30 per cent. **[1664—1599]**

Q. That was the percentage of screenings in the entire lot?

A. In the entire lot; the lumps showed a gain of 1.4 while the fines showed an increase in weight of 8 per cent.

Q. The coarse coal without the screenings showed an increase of 2 per cent, did it not—gain?

A. The mass of the coarse coal, yes.

(The table discussed was here marked Table Number 3, for identification.)

I made certain tests on the oxidation of coal and prepared tables showing the results thereof.

(A tabulation was here produced to illustrate these tests.)

The analytical work was done by a chemist named Mr. Bohart, but he was under my direction. I prescribed the methods and he worked under my direction and we conferred every day as to the results obtained. I made some weighings and Mr. Bohart made some. I made none of the analyses at all.

Counsel for the prosecution having objected to the witness testifying to a table purporting to give the results of experiments, the analyses for which were made by another man, it was agreed finally that the

(Testimony of David M. Folsom.)

witness might testify on the present assumption that the analyses were correct, and with the understanding that counsel for the defendants should hereafter produce as a witness the chemist who actually made these analyses. It was conceded by counsel for the prosecution **[1665—1600]** that the weights involved in said experiments were made by the witness and were as depicted upon the table.)

Mr. OLNEY.—Q. Just proceed now, Professor.

A. The first two tests were made on lots of coal crushed to 10 mesh. These were small lots, one weighing less than a lb.—300 grams. They were placed in an oven where the temperature could be controlled. It was an electric oven, where the temprature could be held at any specific point.

Mr. SULLIVAN.—Q. That is something you don't know yourself, do you? A. Oh, yes, I know that.

Q. Were you present at the time that was done?

A. Yes. This coal was placed in there on the 20th of August and the temperature was set at 45 degrees centigrade, 117 degrees Fahrenheit. A pan of water was placed in the oven with these two samples so that they might have the atmosphere saturated with moisture. Weights were noted on the 27th of August and on the 31st of August were noted again, and on the 31st of August the temperature of that oven was increased to 80 degrees centigrade, which is about 176 degrees Fahrenheit, 36 degrees below boiling. The samples were left in there for 6 days; the final weight of sample No. 1 was 304½ grams, showing an increase over the original weight of about 6 grams.

(Testimony of David M. Folsom.)

The final weight on the second sample was 296.6 grams, showing an increase over the original sample 6.3.

Mr. OLNEY.—Q. When was this final weight taken? **[1666—1601]**

A. It was taken on September 6th. The experiment began on August 20th and the temperature was raised on the 31st of August to 80 degrees. It was exposed to 80 degrees centigrade for 6 days. The moisture determination on the first lot was 2.8 per cent at the beginning of the test and 1.9 per cent at the end of the test; it had lost 0.9 of one per cent in drying out due to the temperature.

Q. In moisture?

A. Yes. In the second lot, the first moisture was 2.9 and the second was 1.7, it lost 1.2 per cent of its moisture due to the drying out. The total gain in weight was 2.04 per cent in one case, and 2.17 in the other. This gain in weight when the moisture content had dropped could only be ascribed to a gain through the absorption of oxygen from the air. To determine whether this oxygen was absorbed by the sulphur or by the coal itself we made an analysis to determine the total amount of sulphur in the coal; the amount of soluble sulphur, that is, the amount of oxidized sulphur, because when the sulphides of iron are exposed the sulphate becomes soluble, and the increase in weight from that cause noted; it amounts to about .04 of a per cent in one case, and .06 in the other; it means that the change in weight in this particular test due to the oxidation of coal itself was 2.9

(Testimony of David M. Folsom.)

per cent in one case and 3.3 per cent in the other.

Q. Was that an increase in weight?

A. That is an increase in weight due to oxidation, the absorption of oxygen from the moisture of the oven. **[1667—1602]**

A sample was then crushed to 60 mesh—two samples—and placed in an air-tight glass vessel in which an extra amount of oxygen could be introduced, and where there was 60 per cent of oxygen in the atmosphere inside of the glass vessel instead of the normal 21 or 23 per cent; that is, about three times as much oxygen as there is in the ordinary air, and some weighing was made occasionally.

Q. What are the dates,—how long did that experiment last?

A. One experiment lasted from the 28th of August until the 8th of September, a period of 10 days, and another one lasted from the 3d of September until the 25th of November, about two months and a half, or two months and 22 days. In the case where the sample was exposed for 10 days you get a total change of weight of 1.4 per cent, a total increase of weight. This also is saturated atmosphere due to a vessel of water being placed with a small lot of coal, but it was kept at ordinary temperature of course and absorbed a certain amount of oxygen from the saturated atmosphere; the change in weight which can be attributable to the oxidation of the coal in this case amounts to only 0.18 of one per cent; while with the lot of coal which is exposed to the atmosphere with an excess of oxygen for two months and 22 days the

(Testimony of David M. Folsom.)

total gain in weight is 2.12 per cent; the gain which is attributable to the oxidation of the coal is 0.76 of a per cent, and the oxidation of sulphide about 0.16.

Q. The total due to oxidation would be 0.92?

A. The total due to oxidation would be 0.92 per cent of the original weight. The other samples were placed in **[1668—1603]** a desiccator where the air was renewed; it was a tight glass vessel, where the air was renewed, but without any excess of oxygen, simply putting in fresh air as the oxygen was withdrawn from the containing vessel. The final gain in weight was 2.35 per cent and 1.58 per cent. The gain attributable to the oxidation of coal in one case was 0.59 and in the other case 0.38; the gain attributable to the oxidation of sulphides was 0.16 in one case and 0.10 in the other; that is, assuming that the sulphide was oxidized to sulphate and took up a water crystallization. The total gain which can only be attributed to oxidation of the sulphide of the coal amounts to 0.75 in one case and 0.48 in another case.

Six lots of coal were crushed and exposed to the atmosphere in porcerlain vessels in a closed room where there would be no draughts, but placed in front of a large window through which the sun would shine; some of the first lot were moistened; and first they were moistened every day, and then later on they were moistened about once a week; these lots could be weighed, the whole dish could be weighed, and the known weight of the porcelain vessels subtracted would give the weight of the coal from time to time; after being exposed, the first one from the

(Testimony of David M. Folsom.)

25th of August until the 21st of November, about three months, the total gain was 0.55% ; the gain due to oxidation of the coal was about 0.17 and the oxidation of sulphide about 0.08. The total gain which could be attributed to oxidation being 0.25 of a per cent, a quarter of a per cent. That cial was just out in the ordinary open air although it was wet occasionally. A second and **[1669—1604]** a similar sample was exposed without wetting, just simply left right out in the air for a period of approximately three months, from the 25th of August to the 21st of November, and gained during that time 0.44 per cent. The gain attributable to the oxidation of the coal being 0.27 per cent, and to the oxidation of the sulphide about 0.07 per cent.

Q. What was the total in that case due to oxidation? A. 0.34 per cent.

Mr. ROCHE.—Q. Within what period of time?

Three months approximately, from August 25 to November 21. These two were crushed to 60 mesh, they were crushed very fine, as fine as dust. Of course, the crushing would tend to accelerate the oxidation somewhat. Here is one that was crushed to 10 mesh, two lots, one exposed from the 23rd of August to the 24th of November, and the other one from the 21st to the 24th, both about three months, and showed a loss of weight in the end, one of them losing in weight 0.6 per cent and the other one a loss of 0.62, practically the same. This loss was due to a drop in the moisture content, the original moisture in one being 2.8, and in the other 2.9, a drop in moisture of about one per cent. The loss in moisture was

(Testimony of David M. Folsom.)

greater than the loss in weight, and I attribute the difference to the oxygen absorbed from the moisture by the coal to the amount of 0.26 of a per cent in one case and 0.22 of a per cent in the other.

Mr. OLNEY.—Q. What was the total gain of weight due to oxidation?

A. The total gain of weight due to oxidation was 0.4 per cent in one case and 0.28 per cent in the other.

Mr. SULLIVAN.—Q. By the way, was all this fine coal **[1670—1605]** that was tested?

A. Yes.

Q. All powdered, pulverized?

A. Yes. The last two tests crushed to 60 mesh and exposed to the sun, moistened, and one without moistening; exposed from the 25th of August to the 25th of November; the coal that was crushed to 60 mesh did not lose as much moisture; it retained some of its original moisture, it didn't have as high moisture to begin with, it had 2½ to begin with, and dropped to 1.9; there was an increase in weight shown there, and a very high increase in weight in this particular case due to oxidation or which could only be attributed to oxidation.

Q. What were the total amounts?

A. About one per cent attributable to oxidation; the total gain was 0.99 in one case and in the other case 1.03 per cent. This coal crushed in this way, in small lots, and wet repeatedly would oxidize more rapidly than coals in large piles except where additional heating took place.

Mr. OLNEY.—Q. Would heating add to the rapid-

ity of the process?

A. The heating increases the rate with which all oxidation takes place. That is one of the factors which controls oxidation.

Mr. SULLIVAN.—Q. What was the temperature?

A. Normal temperature. In the first two the temperature was increased by placing them in electric ovens, but everything after that was normal temperature, room temperature; in certain parts of the tests the weather was warm, up around 90, and at other certain **[1671—1606]** parts it was down around 50.

I visited Nanaimo at the request of counsel for the defendants, about December 23d, 1913, to examine, and I did examine, the scales on which the coal was weighed commercially. I was in company with Professor Parr.

(It was at this point explained by counsel for the defendants that the purpose of the testimony about to be elicited was to show the difference between weighing with an even beam and with an up beam and with a down beam, respectively, on the scales at Nanaimo.) We weighed a train of 20 cars, taking a little over half an hour to make the weighings. First we weighed them with the scale beam above the center, and not quite touching the bar at the top; then we changed the position of the slide on the scale beam until the beam was not quite touching the bar at the bottom; then we had the train of twenty cars run over the scales again, taking approximately half an hour for the determination of the weight and

(Testimony of David M. Folsom.)

weighed said cars using as nearly an even beam as we could obtain. The net weight of the coal when the train was weighed with the beam just below the top was 207,214 pounds, and when the beam was just free of the bottom, 209,566 pounds, a difference of 2352 pounds or a percentage of 1.13 of a per cent of the larger weight. The net weight of the coal weighed on the even beam was 207,666 pounds, being 1900 pounds lighter than the weight with the beam just off the bottom, or a percentage of .94 on the even beam weight. The even beam weight was 452 pounds heavier than the weight with the beam just approaching the top, or a percentage of .22%. The scales at Nanaimo are right on the ground, set in the railroad track. There is very little possibility of vibration. **[1672—1607]**

BE IT REMEMBERED that thereupon the following testimony was given and that the following proceedings occurred:

Q. Did you make similar tests on the scales on the bunkers at Folsom Street? A. I did.

Q. State what you did there?

A. We took during the noon hour and ran a train of four cars over the scales five times, weighing the cars two at a time and noting the weight recorded with the beam just free from the bar at the top, just free from the bar at the bottom, and with the beam even; we did that five times.

Q. What differences did you find between the up-beam and the even beam?

A. We found a difference in both cases of an average of 42 lbs.

(Testimony of David M. Folsom.)

Q. What percentage is that to the weight of the coal?

A. I think I gave the wrong figure—what did you ask?

Q. I asked betwen the upbeam and the even?

A. Between the upbeam and the even beam a difdifference of 22 lbs. in one case and a difference of 23 lbs. in the other case.

Q. And what difference between the down beam and the even beam?

A. Between the down beam and the even beam a difference of 19 lbs. in one case, and a difference of 20 lbs. in the other case.

Q. Did you observe anything about the Folsom Street bunkers that would make it difficult to weigh accurately?

A. The fact that the cars are weighed coupled together makes it difficult to weigh accurately and—

[1673—1608]

Q. (Intg.) By the way, in that connection, in these separate weighings which you made of the same cars at Nanaimo did you always get the same weights on the same car? A. I did not.

Q. By how much did the same car differ in weight?

A. It differed in weight sometimes more than 100 lbs.

Q. I don't want the detail of all of that, Professor, but coming back to the bunkers here at Folsom Street, is there any other cause at Folsom Street which would make it difficult to weigh accurately?

A. The vibration set up by the trains as they are

(Testimony of David M. Folsom.)

running over the track in the ordinary process of handling the coal, and the vibration induced by the dumping of buckets of coal in the pockets over the bunkers, vibration which intensifies the difficulty of weighing and magnifies the difference between up and down beam; also the fact that the bunkers are open and exposed to the wind, and the scales are open.

Q. Did you make any observation as to what the difference would be between an even beam and a beam which was caught as it was going up, without being allowed to poise?

A. We made a number of experiments, first, in sliding along rapidly until the beam rose; we got a difference varying from 30 lbs. up to as high as 70 in some cases between an even weight and the weight when the slide was pushed rapidly until it rose.

Q. And what was the net weight of the coal?

A. About 12,000 lbs.

Q. What difference would there be, in your opinion, and according to your observation, between weighing with [**1674—1609**] an exactly poised beam and weighing with a beam that was caught as it was coming up, that is, weighing in a commercial manner.

Mr. ROCHE.—That is objected to upon the ground that that is not the subject of expert evidence. He has already testified to the actual experiments.

Mr. OLNEY.—The point of the matter is, if the Court please, that there is a difference between weighing with a beam which is just poised below the top of the scale and a weight which is taken as it is

(Testimony of David M. Folsom.)

testified here the weights are taken, with a beam coming up and being caught before it is permitted to poise, the weigher judging of the speed with which it is coming up.

The COURT.—Is that a matter that can be determined by actual observation?

Mr. OLNEY.—I think not. Well, it is a matter as to which you could not go down there and say in any particular case, your Honor, but it is a matter that a person watching it could form an estimate as to what the difference would be.

The COURT.—The objection is sustained.

Mr. OLNEY.—We note an exception. That is all.

(NOTE: The plates and tables used in connection with the examination of the witness Folsom were introduced in evidence as Defendants' Exhibit "SS" at a later point in the trial, viz., at the conclusion of the testimony of the witness, Albert Lockett, *infra.*)

Cross-examination by Mr. SULLIVAN.

I have had no occasion in the last 3 years to use any scales that weighed more than 200 pounds. During the time that I was employed with the Boston & Montana Mining Company, the engineering department to which I belonged, had supervision of scales which were used for weighing ore, copper, and all the materials around a smelter, which scales [**1675—1610**] ranged in size from 100 ton track scales to scales which weigh copper accurately, in about 5,000 pound lots. I was not the weigher there and I have never been a weigher employed about scales. I have never experimented with large scales in this way be-

(Testimony of David M. Folsom.)

fore, but I did watch the scale inspector at his work, because it was a part of my duty to see that he properly tested the scales and that they were in adjustment and in proper condition to weigh. I have never personally tested any scales for any purpose.

I have been employed by the defense since last July. My compensation is $600 a month for the time engaged in actual work. I have been employed since July—probably 4 or 5 months, continuously. My expenses have been paid and the expenses of my assistants, in connection with the work that I have done for the defendants.

I graduated from Stanford University in 1902. From there I went for one year to the Columbia School of Mines. Then I came back to Stanford for one year and next went to the copper mines in Montana. I had had no experience in a coal mine up to that time. I was engaged at the smelter of the copper mines for 5 years and 3 months. About 400 tons of coal a day were brought there for fuel purposes. I had nothing to do with the burning of coal, but I inspected its storage. It was bituminous coal. We kept up to 4,000 tons in the yards there in storage at a time. A tally was kept showing the amount of coal that went into these piles, and of the amount of coal that went out too. These tallies were kept by the clerical force. On the first of every month I had to estimate the amount of coal in the pile for the purpose of striking a balance and **[1676—1611]** checking the weights of the smelter foreman. I made no analysis of coal there at all.

(Testimony of David M. Folsom.)

Q. Did you make a tally or a record of the amount of coal that had been taken from a given pile?

A. I made no such record, but such records were made though.

Q. You saw such records, did you?

A. Yes, sir, I saw them.

Q. Can you recall a single instance where a pile containing a given number of tons ever showed an increase of weight when the pile became exhausted? **[1677—1612]**

After leaving the Copper Company I took up teaching at Stanford. Up to the time I went to Nanaimo, I had never been in a coal mine, and I did not go into a coal mine up there. The company for which I worked in Montana, was the Boston & Montana Mining & Smelting Company, Consolidated. My first position there was in the testing or sampling department. Then I became assistant engineer and afterward field engineer, acting as chief engineer in construction work. During part of the time I had charge of the ordering in of supplies to the smelter, etc. This place was at Great Falls, Montana—the company had an office there. Their main office is in New York. The only experience that I had in the matter of coal there was the ordering of supplies of coal and the measuring of stock. In January, 1910, as I said, I came back to Stanford, and I have been teaching there since in the mining department. We include in that department all kinds of mining, including coal to a slight extent. My duties in relation to lectures or instruction in

(Testimony of David M. Folsom.)

coal are very slight. I usually give two or three lectures a year on coal. Those lectures usually last one hour each. During the last three years I have spent about six hours lecturing on coal. I might add, however, that in the first year I was there I gave 6 weeks' lectures on coal, afterward dropping them because I did not think the students who were going to work on the Pacific Coast had much use for lectures on that subject. The material that I used in preparing those lectures was to a great measure derived from books. I have never had any practical experience in coal. It is not true that my knowledge of coal is **[1679—1614]** purely theoretical. It is derived from text-books, from talking to coal miners and engineers. It is the common experience which any engineer might have. I have never made an analysis of coal except I may have when I was a student. We made an analysis simply of best units contained in coal. I have never made an analysis of coal for the purpose of determining the amount or increase in the weight of coal by reason of oxidation or for the purpose of determining the moisture content of the coal, or for ascertaining the change in the weight of coal from any cause—except, of course, in the tests that I have described here in court.

I never in all my life previously made such analyses as those on the tables which have been shown here, which tend to show a certain reduction in the moisture content in coal, and a certain increase in weight by reason of the oxidation of the sulphur in

(Testimony of David M. Folsom.)

coal and a certain increase in weight by reason of the oxidation of the substance of the coal.

Q. Prior to your work shown upon these tables, here, had you ever made any tests of coal for the purpose of determining the increase or decrease in the weight of coal by reason of oxidation?

A. I never made any tests on coal, no, sir.

Q. Had you ever before in your life made any tests for the purpose of determining the moisture content of coal, or the loss by reason of the moisture content of coal, or gain by reason of the moisture content of coal?

A. I never made any tests to determine the moisture content; I made tests for the purpose of determining the change of weight in ore.

Q. But up to the present time you never have made any analyses or any tests for the purpose of determining, in the first instance, the amount of moisture in a given amount of coal; that is a fact, is it not?

A. If I understand the question.

Q. That is a fact, is it not?

A. It is. **[1680—1615]**

Q. Up to this time you never have made any tests with coal soaked in water for any given number of days, for the purpose of ascertaining the increase in weight of coal by reason of that soaking in water?

A. Not before these tests.

Q. Before these tests, you never had done it?

A. No, sir.

Q. Before these tests, had you ever made any experiments with coal for the purpose of ascertaining

(Testimony of David M. Folsom.)

the amount of moisture resulting from exposure to rain for a given number of days? A. No, sir.

Q. Or did you ever before these tests make any experiments for the purpose of determining the amount of moisture that would accrue by reason of a saturated atmosphere surrounding coal?

A. No, sir.

Q. And before these tests, did you ever make any experiment with pulverized coal for the purpose of ascertaining how it would behave under increased temperature? A. No, sir.

Q. Or under ordinary temperature? A. No.

Q. Who made these beautiful plats here, from first to last, do you know? A. Yes.

Q. Who made them?

A. The first one was made by a man by the name of R. W. Whittaker, and a man named Kenneth Pyle, two men worked on them; most of them were made by Pyle.

Q. Did you, with your own hand, make a single figure upon any one of these beautiful plats that have been presented here to the jury? A. Yes, sir.

Q. Which one?

A. I made a part of the last one. **[1681—1616]**

Q. Where is your work upon this?

A. I made part of the letters, I think.

Q. You mean Table 1 and Table 3?

A. Yes, I made those.

Q. What else did you do?

A. This last one was made very hurriedly, Mr. Pyle was sick, or I would not have touched this one.

(Testimony of David M. Folsom.)

Q. If Pyle had not got sick on the job, you would not have touched this one yourself?

A. No, sir, I would never have done that work.

Q. Did you make any of these figures here upon this table?

A. No, I didn't make any of the figures.

Q. So, then, the only work you did upon any of these plats here is the printed matter appearing upon the second column, showing the treatment to which this coal was subjected; is that so?

A. That is all the work that I did, yes, except that I stood over them and watched them doing it.

Q. That is, you did the treatment work here on the plat?

A. I think I am responsible for some of that.

Q. Did you suggest to Mr. Olney and these other gentlemen, when they called upon you to do this very fine work, that they ought to have employed some man who had had some experience in this kind of work, so that the work could be accurately done?

A. To the best of my recollection I told Mr. McCutchen when he came to see me that all I knew about coal was that it was black, and that it would burn under certain conditions, but that I was perfectly competent to make these tests that he wanted made. **[1682—1617]**

In making these tests and analyses I had Mr. Bohart assisting me. He did the analyses and looked after the actual work shown on the tabulation relating to tests of oxidation. All of the other work was my own except occasional weights when I was absent,

(Testimony of David M. Folsom.)

which were taken and reported to me. The work that was done in my absence was done by H. W. Young, and it consisted of the weights taken in the various experiments between December 20 and December 27, 1913. There was one weight a day taken on two or three cans of coal which were exposed. No other parties assisted me in my experiments except the men employed for the manual labor.

The first of the experiments which I have described on direct examination began on August 14, 1913. I had, however, performed some preliminary experiments before that time on coals which had been sent to me at Stanford University, by the Western Fuel Company. These experiments began on July 19th, and there were some 20 or 30 of them altogether.

There were seven different kinds of coal sent down to me and I made three experiments on each lot for the purpose of determining the change in weight under ordinary atmospheric conditions,—to determine the change in weight from soaking the coal in water, and to determine the change in weight from sprinkling. The first experiment was for the first mentioned purpose. I have one sheet here which is a tabulation of the results secured therefrom.

Q. Will you let me look at it?

Mr. McCUTCHEN.—One moment, if your Honor please. We submit that counsel has not the right to look at it, he can examine the witness on it, if he wants to develop that experiment. There has been no evidence offered with respect to it here up to this time. We are perfectly willing that counsel should

(Testimony of David M. Folsom.)

go into it, but I do not understand that counsel has the right to [1683—1618] call upon the witness to surrender a paper which the witness has in his possession and, after counsel, has examined it, toss it to one side, as he did yesterday, when he called upon a witness for a paper. The experiment is not yet in evidence.

The COURT.—He may desire to offer it in evidence.

Mr. SULLIVAN.—We (may desire to) offer it in evidence.

Mr. OLNEY.—Exactly, your Honor. It seems to us it is not quite fair for counsel to take up a report, read it over, and then ascertain whether he desires to put it in, or not. He has to find that out on cross-examination, and not by means of an inspection of the document previously, and practically as if the witness were not on the stand.

Mr. SULLIVAN.—We submit, if your Honor please, we have the right to examine the report, and we have the right afterwards, if we care about it, to introduce it in evidence.

The COURT.—It seems so to me. The objection is overruled.

Mr. OLNEY.—We note an exception.

Mr. SULLIVAN.—Q. This paper purports to be a report of test No. 1, to determine the change in weight under ordinary atmospheric conditions, of date July 21, 25, July 26, 28, August 21. I ask you how long the coal which you received was subjected to ordinary atmospheric conditions?

(Testimony of David M. Folsom.)

Mr. McCUTCHEN.—I object to that, the paper is not in evidence, and counsel is cross-examining the witness upon it.

The COURT.—There is some force in that. If you want to ask him any question, he ought to have the paper in his [1684—1619] hand. I don't know what the paper is, but I suppose it discloses the facts you want to know.

Mr. SULLIVAN.—We will offer it in evidence as a part of the cross-examination of the witness. I will let the witness have it.

Q. What does that paper purport to be?

A. A tabulation of a test to determine the change in weight under ordinary atmospheric conditions, the change in weight of coal, small quantities.

Q. Don't you remember how long that coal was subjected to the ordinary atmospheric conditions?

A. One month.

Q. Between what dates?

A. Between July 21 and August 21.

Q. Between July 21 and August 21?

A. Yes, sir.

Q. What was the quantity of coal that was subjected to this test?

A. About 11 pounds; well it ranges from about 9 pounds up to about 12 pounds, as a maximum.

Q. State what kinds of coal were tested by you in accordance with that report.

A. These coals came to me labeled "Castlegate," "Comax," "Richmond," "Black Diamond," "Japan," "White Ash," and there was one sack on

(Testimony of David M. Folsom.)

which the label was lost.

Q. Was the sack on which the label was lost, was that Nanaimo coal?

A. I have no idea what it was; I am sure it was not Nanaimo coal, it was domestic coal, I think.

Q. What is the character of the coal, so far as coarseness or fineness was concerned—of that coal which you experimented with?

A. It ranged in size from lumps about 6 inches in diameter down to dust.

Q. A mixed coal, was it?

A. Yes. **[1685—1620]**

Q. What percentage of screenings was in the samples? A. I didn't screen any of this coal.

Q. About what quantity of screenings would you say were in the coal?

A. Oh, estimated at about between 15 and 40 per cent.

Q. Between 15 and 40 per cent; what do you call screenings?

A. Using the term that I used before, under one-half inch in diameter.

Q. You made the test, as I understand you; how did you make the test?

A. I simply placed approximately ten pounds of coal in a shallow pan, something like a milk pan, it was galvanized iron, I placed it outside on top of a table that is about 8 feet above the ground; the coal just about filled the pan.

Mr. OLNEY.—Q. How deep was the coal in the

(Testimony of David M. Folsom.)

pan? A. About 2 inches.

Mr. SULLIVAN.—Q. Did any rain fall during the time this coal was subjected to the atmospheric influences?

A. A little rainfall, .09 of an inch.

Q. What did you ascertain when you made that experiment, as to the loss or gain of that coal which you experimented with to show the result of the atmospheric conditions for one month?

A. I found that immediately after the rain, a day after the rainfall, the rain fell for two or three nights in small showers, between July 21 and the 25th; on the 26th of July, and on the 25th of July the coal showed a gain in weight, various samples, it ranged between .093 per cent gain up to .3 of a per cent gain. This coal was exposed until August 21st, and most of the samples had lost [1686—1621] its moisture.

Q. Now, as a matter of fact, during this one month's exposure, and notwithstanding the downfall of rain, all of this coal diminished in weight, did it not, according to your own report?

A. Yes, sir, but that is not a fair test—

* * * * * * * * *

because the wind was blowing quite strongly down there during the month of August, and it blew out a good deal of dust from the pans, and I didn't pay much attention to that test, except that it indicated that coal was susceptible to change in weight.

Q. Is it not the same kind of wind that blows down there that blows over the Folsom Street bunkers up

(Testimony of David M. Folsom.)

here? A. Yes, sir.

Q. Is that the only reason you can give for the result of this experiment of yours?

A. Oh, no. It was a small lot of coal. Moisture would evaporate very quickly from a depth of two inches; it would evaporate from a depth of a foot, approximately.

Mr. SULLIVAN.—I will read this in evidence to the jury.

"TEST No. 1.—To determine the change in weight under ordinary atmospheric conditions.

Date.	July 21.	July 25.		July 26.	July 28.	Aug. 21.
Name of Coal.	Original Moisture.	Original Weight.	Weight.	Grain.	Weight.	Weight Referred to Original Weight.
Castle Gate	2.58	4934 gms.	4996 gms.	1.5%		

Q. I will ask you if that gain of 1½ per cent was not due to the fall of rain which occurred between July 21 and [1687—1622] July 25.

A. To the best of my knowledge.

Q. (Continuing reading:) "Castle Gate 2.58 4934 gms. 4996 gms. 1.5% 4951 gms. 4934 gms."

Comax. It shows no loss at all, as I understand it; is that correct?

A. Well, you have the paper, I think that is correct.

Q. Just look at it. A. No loss at all.

Q. And shows a loss of 62 grams since the 25th day of July, does it not? A. Yes.

Q. Oxygen does not seem to have had much effect on that particular lot of coal, does it?

(Testimony of David M. Folsom.)

A. It is impossible to determine anything about that under the conditions of that test.

Q. Now, here is Comax coal, July 21, original moisture, 1.16 per cent; original weight, 5463 grams; July 25, 5587 grams, showing an increase of 2.27 per cent. That increase of 2.27 per cent was due to the downfall of rain, was it not, between July 21 and July 25? A. I take it that it was, yes, sir.

Q. And July 26, 5495 grams, July 28, 5468 grams, August 21, just a month afterwards, 5468 grams; is that a minus .03 of 1 per cent?

A. Minus .03 of 1 per cent.

Q. In other words, that shows a loss, does it?

A. That shows a loss, yes, sir.

Q. The next coal on the list is Black Diamond: Original moisture 9.72, original weight 4871; July 25, 4983 grams. That is after the rainfall, a gain of 2.31 per cent. That was due to rainfall, was it not?

A. I take it that it was.

Q. July 26, 4947 grams; July 28, 4887 grams. There was a slight gain there. Japan, original moisture, 2.24; July 21, original weight, 4162; July 25, 4214, showing an increase of 1.25 per cent in gain. That was due to rainfall, **[1688—1623]** was it not? A. I take it that it was.

Q. July 26, 4170 grams; July 28, 4162 grams; weight 4131 grams, showing a loss of minus .08 of 1 per cent; that is correct, is it not? A. Yes.

(The preliminary report herein above last testified to by the witness was marked U. S. Exhibit 164, and reads as follows:)

[U. S. Exhibit No. 164—Preliminary Report.]

TEST No. 1.—To determine the change in weight under ordinary atmospheric conditions.

Date.	July 21.	*July 25.	July 26.			July 28.	Aug. 21.	
Name of Coal.	Original Moisture.	Original Weight.	Weight.	*Grain.*	Weight.	Weight.	Weight.	Referred to Original Weight.
Castle Gate	2.58	4934 gms.	4996 gms.	1.5%	4951 gms.	4934 gms.		
Comax	1.16	5463	5587	2.27	5495	5468	5468 gms.	.03%
Richmond	2.03	5579	5631	.93	6588	5579	5564	.03
Black Diamond	9.72	4871	4983	2.30	4947	4887		
Japan	2.24	4162	4214	1.25	4174	4162	4131	.08
White Ash	6.52	3726	3806	2.15	3763	37.10		
No. Name	2.72	4700	4788	1.87	4714	4696		

* .09 of an inch of rain fell between July 21 and 25th. August warm with low humidity.

TEST No. 2.—To determine the gain in weight resulting from soaking the coal under water.

Date.	July 22.	July 23.		July 24.	
Name of Coal.	Original Weight.	Wet Weight.	Gain.	Wet Weight.	Gain.
Castle Gate	6791 gms.	6972 gms.	2.66%	7018 gms.	3.34%
Comax	8438	8905	5.54	8913	5.63
Richmond	7949	8127	2.24	8158	2.63
Black Diamond	8230	9145	11.12	9215	11.97
Japan	7369	7554	2.51	7590	3.39
White Ash	7686	8416	9.50	8420	9.5
No Name	8603	8853	2.92	——	——

[1689—1624]

(Testimony of David M. Folsom.)

The tests made from July 21 to August 21, 1913, do not include a test on Wellington coal, except that one of the 7 varieties of coal that I used then was stated to be Comax coal. I made no test then on Wellington coal to determine the increase in weight thereof resulting from oxidation, nor have I made such a test on Wellington coal at any time. Nor did I a short time after this July to August test make any exposure of Wellington coal. I did not get any Wellington coal until some time in October.

Q. Does not the Wellington coal contain a less proportion of sulphur than any one of these seven different varieties that were exposed as shown by your report here during the month of July and August?

A. I only know the relation between the sulphur contained between the Wellington and the Richmond coal, it is approximately the same.

Q. Is not the sulphur content of Wellington coal less than of Richmond coal?

A. It is approximately the same.

Q. The chief cause of the oxidation of coal is due to a combination of the oxygen of the air with the sulphur of the coal, is it not? A. No, sir.

Q. Well, what is the chief cause of the oxidation?

A. The absorption of oxygen from the moisture by the unsaturated hydro-carbons that exist in the coal.

Q. Does not sulphur also absorb the oxygen?

A. Yes, sir. [1690—1625]

Q. Does not the sulphur proportionately absorb

(Testimony of David M. Folsom.)

more oxygen than the substance of the coal itself?

A. Proportionately it does, but there is not very much sulphur in these coals to absorb oxygen. High sulphur coals would take up more oxygen than the coal itself would absorb. In the Wellington and the Richmond coals the chief cause of oxidation would be the absorption of oxygen by the coal itself.

I am not a chemist by profession. It requires a man who is in practice and doing analytical work right along to perform accurately these analyses that I have described. I have not made analyses of coal myself. I have not done any analytical work of this kind. The analyses presented on the tables that I have shown in court were done by George S. Bohart, a professional chemist employed at Stanford University.

Q. Now, Professor, what is the actual cause of oxidation, as you understand it?

A. I am not sure that I understand your question.

Q. I say what is the cause of oxidation—how is it produced?

A. Oxidation is simply a combination of a substance with oxygen in chemical form; it is a combination such as takes place constantly. All sorts of substance absorb oxygen.

Q. The human body is oxidizing, is it not?

A. It is.

Q. Continually? A. Continually.

Q. The air that we breathe produces oxidation in the body, does it not? A. It does.

Q. And while a man who is suffering say loss

(Testimony of David M. Folsom.)

of weight, he is continually subjected to oxidation, is he [1691—1626] not?

A. He is; nevertheless, oxidation causes an increase of weight of the material oxidized.

Q. Does it cause an increase in the weight of a man who is oxidizing continually?

A. No, that may not be.

Q. So that a man who weighs 150 lbs. to-day, what would be the increase in weight by reason of oxidation a year from to-day, would he weigh 1000 lbs. more or less, that is, the oxidation continues to cause an increase in weight?

A. The man may not weigh as much, but the oxidation is causing an increase in weight, nevertheless.

Q. Assume that instead of a piece of coal, you take a man—he is oxidizing terrifically, is he not?

A. Yes.

Q. But at the end of the year the 150 lb. man may not weigh 135 lbs.; is not that so? A. Yes, sir.

Q. And is not the reason for that, Professor, that while the process of oxidation is going on there is coming from the body or from the coal, other gases which are formed in the body, and a certain amount of waste is going on?

A. That is true so far as the man is concerned, yes.

Q. Decaying vegetables are the result of oxidation, are they not? A. Yes, sir.

Q. And decaying animals are brought to a condition of decay by reason of oxidation?

A. Yes, sir.

(Testimony of David M. Folsom.)

Q. Take the case of a dead man, weighing 150 lbs., the process of decay is nothing more or less than a process of oxidation, is it?

A. That is what it is.

Q. Assuming the process of oxidation goes on in the dead man for a year, do you mean to say that at the end of the year that dead man will weigh more than 150 lbs.? [1692—1627]

A. No, I don't mean to say it.

Q. Now, as a matter of fact, does not the oxidation cause a decrease in the weight which ultimately results and a destruction of the man himself, or the separation of all the contents of the man?

A. It leads to the separation of the man, yes, but what causes the increase in weight of the chemical—the chemical substance in the man, increases in weight but are dissipated.

Q. Instead of a lump of coal just assume that you had a dead man who was decaying by reason of oxidation, and assume that after 60 days you pulverized that part of the corpse and made an analysis of it, would you come into court with a table showing an increase in weight resulting from the oxidation of that dead man?

A. It would be perfectly possible to come into court with such an oxidation provided that all the products of the oxidation were saved.

Mr. SULLIVAN.—You say it would be possible. Of course, we admit the possibility of it. Now, Professor, is it not a fact that while the process of oxidation is going on there is another process going on

(Testimony of David M. Folsom.)

at the same time which more than offsets any possible increase resulting from the oxidation?

Mr. OLNEY.—You refer to coal?

Mr. SULLIVAN.—I refer to anything in the process of decay resulting from oxidation.

A. That is too general a statement.

Q. Can't you answer the question?

A. If you mean in the case of material undergoing decay, such as a dead man, which you have used for the purposes of illustrating, or decaying vegetables, the oxidation of [1693—1628] the material causes an increase of weight, but the oxidation products are dissipated. Now, in the case of coal the oxidation is simply an absorption of certain hydro-carbons in the coal which are not saturated with oxygen; it is drawn in, absorbed from the air, absorbed by the body of the coal; there is no decaying of the coal in the same sense that there is a decaying of vegetable matter.

Q. There is a decaying, is there not—is there not combustion, a slow combustion—that is what oxidation is, is it not, a slow combustion?

A. There is a very marked difference in degree between the two. It might be termed a very slow combustion, but it is not a decay; there are no products given off.

Q. No products given off?

A. By this state of oxidation.

Q. Well, take the case of a cabbage head weighing about 10 lbs. Now, that is exposed to the air, that decays by reason of the oxygen of the air combined

(Testimony of David M. Folsom.)

with the elements of the cabbage; is not that so?

A. Yes, sir.

Q. At the end of six months there would be very little cabbage left, would there not?

A. Well, I could not say as to that.

Q. You could not say; you are not an expert on cabbage heads? A. Not yet.

Q. So in 30 days would that cabbage head increase in weight by reason of oxidation?

A. I could not say; I would have to make the experiment.

Q. Would there be anything of the cabbage head left after 30 days of oxidation?

A. I could not answer that question without making the experiment.

Q. Don't you know, Professor, that in the case of all **[1694—1629]** substances it is simply a question of time when oxidation destroys the substances themselves, or at least disintegrates the substance, causing the substance to vanish in air or in gases?

A. That is too general a statement for me to subscribe to.

Q. Now, take the case of a piece of coal, a lump of coal, Professor, weighing say 10 lbs., and assume that that is left in the open air left to the influences of the air for say a period of ten years, will you say that that lump of coal has increased or has decreased in weight?

A. It depends a great deal on the condition of storage.